

# Psychological Review

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# THE PSYCHOLOGICAL REVIEW

## PREDICTION IN CLINICAL PSYCHOLOGY AND BEHAVIOR THEORY

BY G. RAYMOND STONE

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The recent attempt by Shaw (11) to demonstrate a common frame of reference for clinical and experimental psychologists in terms of their common interests in prediction touches an extremely significant point in methodology. That this common interest, if clearly recognized, could work to co-ordinate the efforts of the two groups is a fundamental insight. Appeals to methodology often result in such insights. Shaw admits that the common interest is not clearly recognized and he further suggests that differences in analytic level (molar-molecular) have obscured the fact.

If the process of analysis is conceived as dimensional, varying inversely with subject matter complexity, then the "level" distinctions tend to be supplanted by distinctions in the amount of control, including the controls of isolation (12). As the degree of control increases, the degree of quantitative precision also increases, and as precision increases, the possibility of refined prediction increases. The more precise the data of analysis, the greater the ease of establishing quantitative theoretical postulates from which rigid predictions (deductions) can be made. The converse of these generalizations is also true: The more complex the data

of analysis and the fewer the controls, the more general do the theoretical postulates become and the grosser the predictions from them. The more general the postulates and the grosser the predictions, the more likely it is that the crucial logical rule of valid prediction will be violated; the rule: *Postulates must be stated in such a way that predictions (deductions) from them allow for the operations of both confirmation and failure of confirmation (1, 2).*

It is the present contention that many clinical psychologists and some experimental psychologists violate the rule. It is here that the crux of the matter rests. It is only with predictions which meet this rule that we can expect co-ordination among psychologists of any persuasion. Here is where the common interest *must* be. Only under these conditions does prediction exist in its scientific sense.<sup>1</sup> There are, of course,

<sup>1</sup> The writer has known some charlatans who have made prediction after prediction and each in its turn was confirmed. The charlatan in each case was also confirmed in his own secret powers which made this possible. At the mere suggestion that his predictions could have been nothing but confirmed, he would scornfully deride the critic's narrow experimental bent, the sacrifice of science to controls which destroyed the subject-matter of investigation, and end triumphantly after a few more irrelevancies with some elaborately nasty remarks about rats and religious cows and ivory towers. No amount of persuasion could convince him that the argument was a

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other kinds of prediction. Consider the fortune teller, the penny weighing-machine on Main Street, or the race track tout. It is not just an interest in prediction—any prediction—that can close the acknowledged gap between clinicians and experimentalists. It must be the common interest in *valid* prediction.

This may seem to be a didactic belaboring of a point which should be second nature to the readers of this JOURNAL, but a long list of particulars would be very easy to draw up. Surprisingly little of psychological theory would remain unscathed.

Shaw has suggested that when a clinical psychologist selects a particular therapy he is setting up an hypothesis in the same sense as the experimentalist does in his research. He is right in part. In either case it is the burden of the investigator to *test* the hypothesis. Test in this sense requires that the hypothesis be so stated that the operations not only of confirmation but also of failure of confirmation are possible. A successful prediction is not a valid confirmation of an hypothesis if the latter is so stated that all predictions from it cannot help being successful. A proof is not a proof if there was no possibility of a disproof. If a postulate of a therapy (premise of an hypothesis) says: This therapy can help those who

can help themselves,—how would it be possible to empirically *test* the therapy (hypothesis)?

The real hypothesis that is made when one selects one therapy rather than another is that the selected one is the better therapy. This is capable of controlled testing. Even the null hypothesis that the selected therapy is no better than any therapy at all is one capable of being rejected. The only requirements are those of control and precision. The commonly held belief in some quarters that therapeutic procedures are by their very nature uncontrollable, that controls would destroy the dynamic essence of the procedure itself, reflects an abysmal ignorance of what is meant by control. A process can easily be controlled by a process. Dynamic change, directive or non-directive, can be controlled as it is every day in learning laboratories. Fields of force, rapport, reflections of feeling, transferences, etc. are controllable if they are definable. Control does not necessarily mean constancy; as a matter of fact one of the crucial areas of control in any experiment is that over the independent *variable*. Any procedure without control, therapeutic or otherwise, is a procedure without precision, and as such it is an impossible source for testable hypotheses.

Most current therapies are more than simple procedural hypotheses. They are accompanied by a tremendous overload of conceptual theory which is usually just short of the breadth of total systems of behavior. To use successes in operational therapy as confirmations of the total theoretical structure multiplies the logical dangers a millionfold. The larger theory is usually crammed with untestable and gratuitous postulates, ones which in a deductive system could in combination predict anything no matter how the data happened to fall. This is a danger of any large deductive theory and is by no means limited

logical one and that his replies begged the question. I have since become adjusted to this state of affairs when talking to charlatans. They have, after all, a deep-rooted anti-science attitude and a firmly cemented belief in their own secret powers that no amount of persuasion could change one iota. The frequent picture of the charlatan as a crafty and intelligent deceiver motivated entirely by mercenary considerations needs to be changed. I have found them dull, vain and sincere, occasionally with doctor's degrees, frequently poor, but invariably possessing a composure which could withstand any circumstance except that of another claiming to possess the same secret powers. Then character assassination would set in.



to those theories behind therapies. If Freud (4) had both his life and death instincts, so did Pavlov (9) have his excitation and inhibition, to say nothing of his inhibition and disinhibition. Hull (7), too, had been plagued (6) by the excessive predictability of his afferent neural interaction postulate, Guthrie (5) by his movement-produced stimuli, and Thorndike (14) by his neurones. It is not easy to find possible negative cases in these theories. Tolman (15) ruefully admits that he has found, to his cost, that Hull's S-Rs can explain (predict) anything. If Hull does not reciprocate this feeling with respect to Tolman's cognitions, Spence (13) in all probability would. Estes' (3) suggestion to the student reviewing Mowrer's (8) theoretical statements that neutral symbols be substituted for conclusion-laden terms is indeed a good one to test for *ad hoc* explanation. Most large psychological deductive theories are not sophisticated or precise enough even to commit the fallacy of affirming the consequent.

The writer will venture to suggest at this point that never has a theory been constructed by the mind of man which could not predict with some successes. This includes Christian Science as well as science. Theories (or therapies) are frequently defended because they "work," but all theories (therapies) work in this sense. The scientific question as to whether a theory or therapy produces new knowledge or explains present knowledge must be answered by appeals to testable *infirmation* (10) as well as to confirmation.

The suggestion of concentrating attention on the problem of prediction in order to coordinate psychological endeavor is indeed an excellent one. There is little reason to believe that it will automatically make psychologists agree, but it does have the advantage of keeping investigators closer to the methods which characterize scientific advances. In this sense prediction is

understood to be a strict methodological term. After all, "all men are mortal" is a very bad postulate for an hypothesis which needs empirical testing (science) no matter how good it might be in a rational syllogism (philosophy).

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## A NEW INTERPRETATION OF FIGURAL AFTER-EFFECTS

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Certain forms of figural after-effect have been reported by Verhoeff (35) and by Gibson and his collaborators (16, 17, 18), but it is in the writings of Köhler (22) and Köhler and Wallach (24) that this effect is given its most elaborate phenomenological description and theoretical bearing. Köhler and Wallach felt impelled by their observations to postulate non-neural electrical field processes in the visual cortex. These processes "satiates" the medium in the immediate neighborhood of the cortical representation of a figure inspected over a prolonged period, thus modifying the medium for a subsequent test figure. As Köhler and Wallach themselves point out (24, p. 322), their view is incompatible with contemporary conceptions of how the central nervous system functions, but they believe these conceptions must be changed "because many activities of the nervous system are relationally determined in a way which we cannot understand in terms of separate actions within the anatomical elements," the figural after-effect being a case in point. Since figural after-effects and current explanations of them have considerable theoretical significance, it seems reasonable at this time to subject them to careful scrutiny. The purpose of this paper will be to demonstrate that figural after-effects can be accounted for within the bounds set by generally accepted neurophysiological principles. It will be our thesis that these effects

are due to differential adaptation within the projection system, produced by the prolonged inspection of contours.

### THE PHENOMENON

Since most readers will be somewhat familiar with previous writings on this subject, only brief description of typical effects and the method of obtaining them will be given at this point. As the analysis proceeds—following presentation of the Köhler and Wallach theory and our reinterpretation—further demonstrations of a more critical nature will be studied. The general procedure used to obtain after-effects is as follows: One figure (inspection or *I-figure*) is observed for several minutes with constant fixation (on the point marked *X* in subsequent diagrams). Then, as soon as one stimulus card can be replaced with another, a second figure (test or *T-figure*) is observed and its phenomenal characteristics reported immediately. Figure 1 gives a typical example. Objectively, the two T-squares are equal in size, brightness, distance from *X* and so on, but both are somewhat smaller than the I-square. The fixation point is so placed that the left-hand T-square falls within the contours of the previously inspected I-square and nearer to its right contour. Phenomenally, the left-hand T-square appears *smaller* than the right-hand one, it seems *displaced* away from *X*, its borders appear *paler*, and it may seem to be *farther away* in three-dimensional space. Not all of these characteristics need appear to a given subject at a given time, suggesting that attitudinal factors play a role in such observations. It should be pointed out that the same

\*The writers wish to express their gratitude to Mr. George Suci who has contributed both to the preparation of this paper for publication and to its development in our thinking.

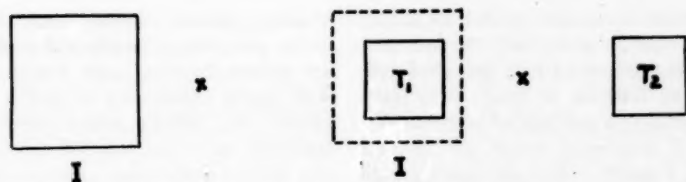


FIG. 1. Typical conditions for demonstrating figural after-effects: *I*, inspection contour; *T*<sub>1</sub>, affected test contour; *T*<sub>2</sub>, comparison test contour; *X*, fixation point.

types of after-effects can be observed with black outlines on white ground, with white outlines on black ground, and for solid figures as well as outlines. This makes it clear that, although prolonged inspection of the I-figure has modified the receptive medium in some manner, no simple fatigue explanation will suffice (otherwise the effects of bright figures on dark grounds should differ significantly from those of dark figures on bright grounds). Furthermore, any retinal locus of these effects must also be dismissed, since I- and T-figures can be presented to different eyes and the same results obtained.

#### THE KÖHLER AND WALLACH THEORY

According to Köhler and Wallach, some region of the central visual system must be conceived of as a quasi-homogeneous volume of tissue through which electrical currents can flow. Presumably this region is area 17 of Brodmann, although neither this identification nor any other is definitely made. These currents are supposed to follow paths of least resistance which are independent of anatomical pathways. The following physical analogy is offered: Imagine a dense and regular network of thin wires filling a three-dimensional space. These wires are everywhere the same in size and other characteristics, are the same distance from one another, and have the same resistance per unit length. They connect as the corners of small cubes. Now, if

a battery is set within such a homogeneous system, the current will flow out from one plate, close about the battery (following the shortest routes), and back into the other plate. However, the flow of current through those wires representing the shortest path will heat them, thus raising their resistance and forcing the current to move outward to wires farther removed from the source.

Although temperature is not raised significantly when currents flow in organic tissues, the same increase in resistance to the further flow of current can be produced through polarization of membranes. However, changes in electrotonus through polarization occur in a few milliseconds, not over the comparatively long periods required for the "satiation" of a tissue area. Köhler and Wallach (24, p. 321) refer to further electrotonic alterations that progress through minutes rather than fractions of a second, but they agree that the physical nature of these changes has not been clarified. Assuming that "standing" potential energy is available in such a volume of quasi-homogeneous tissues, it is postulated that the arrival of the pattern of impulses representing the I-figure serves to disrupt the balance and sets up a flow of direct current. This current takes the shortest path, which lies about the contours of the I-figure, and in doing so gradually increases the resistance of the tissues through which it flows. The increased

resistance forces the current to detour into neighboring regions, displacement occurring according to a negatively accelerated function of time. This process results in a gradient of satiation (increased resistance) about the contour of the I-figure. Since, in analogy with the heating of conductors, tissues do not immediately become depolarized, these satiation effects persist after the I-figure has been removed and can be measured by observing the distortion of subsequent T-figures. In other words, the flow of current representing a subsequent T-figure will necessarily detour about heavily satiated regions of the medium, giving rise to size and displacement phenomena and (not so clearly) brightness and distance effects.

There are numerous questions raised by a theory of this sort. One wonders how such non-neural electrical effects eventuate in behavior, i.e., in the initiation of impulses in motor fibers. At some point along the line we must transfer back from direct currents in a field to impulses in nerve, since this is the way muscles are innervated—but nothing is said about this in the theory. And why is the elaborate anatomical differentiation of sensory cortical tissues necessary? As a matter of fact, the Köhler and Wallach theory would apply just as well if the peripheral projection system terminated on a field of simple cell membranes enclosing electrolytic fluids—the fact that the field is composed of *nerve* tissues is superfluous. The problem highlighted here is the wan controversy between so-called “switch-board” theorists and “field” theorists. Without plunging into this controversy—one which has been characterized by rather extreme positions, at least until the recent and refreshing analysis by Hebb (21)—it would be wise to review briefly certain relevant elementary facts.

All behavior is mediated by certain

sensory, certain central, and certain motor processes. The central processes are presumably the more complicated, with neural *connections* of such a complexity that *strictly neural* “field-like” influences are to be expected and should not appear surprising. Reverberatory neural activity is an established fact as are diffuse central neural connections (26, 27). Activity along sensory neurons exerts its primary influence upon the excitability of neurons with which the sensory elements have axone-soma synaptic connections, and this influence is presumably exerted through a measurable electrotonic potential established at the synapse and decreasing logarithmically as a function of distance from the ending of the active neuron (14). It has never been possible to raise the excitability of adjacent neurons to the level of a propagated nerve impulse in the absence of a synapse-like situation. Only with an artificial synapse (ephapse) has such been possible (2). Such facts as these emphasize the need for further penetrating experimental and theoretical analysis of factors influencing excitability in a *network system* of neural connections.

With respect to figural after-effects, the critical issue is this: Is it necessary to postulate an entirely novel set of non-neural electrical forces in the visual brain? Since figural after-effects cannot be explained in terms of known *peripheral* mechanisms, Köhler and Wallach apparently feel it is necessary. However, neurophysiologists have been steadily pushing the region of known functions back from the retina toward the cortex, and it may prove possible to account for these phenomena in terms of established central mechanisms. Certainly, if figural after-effects can be interpreted without new assumptions about brain action, this would serve the interest of parsimony. Köhler and Wallach open the possibility of

alternative explanations themselves in the following statement:

"Any hypothesis which fits the facts in this field will have the same implications. For, figural after-effects are established only when somewhere in the tissue the level of activity *varies* from one place to another, as it does at a contour or an outline. It follows that, apart from events within individual neurones and chains of neurones, differences in a transverse direction must have some specific effects" (24, p. 323).

Just such transverse differentials of neural activity in the higher centers have been described in considerable detail by Marshall and Talbot (29)—in specific connection with the resolution of contours—and it is the point of view of the present writers that figural after-effects can be interpreted along the same lines.

#### THE MARSHALL AND TALBOT ANALYSIS OF CONTOUR RESOLUTION

A great deal of neurophysiological evidence has been accumulating in recent years which severely complicates classic notions of projection from retina to visual cortex (and equally complicates, therefore, any simple conception of isomorphism). We may start with the general observation (25) that neurons are not detonated as a rule by stimulation of a single bouton, but rather require the arrival within short intervals of time of several impulses over one or several boutons. Observations on the recovery cycle at geniculate and cortical levels (8, 28) have provided evidence for *vertical summation*, which provides for a statistical "peaking" of excitation-frequency in the higher centers. Reciprocal overlap of synaptic connections at several levels of the projection system provides for *lateral summation*. As Marshall and Talbot put it:

"The principle of reciprocal overlap has long been recognized, but only recently has direct evidence become available from the application of silver degeneration techniques. . . . In the cat, optic tract endings in the geniculate divide into several branches and as many as 40 ring-shaped boutons have been seen on single radiation cells which may come from as many as 10 optic tract fibers. Each fiber also divides to form synapses with several radiation cells" (29, pp. 121-122).

Such a system provides for multiplication of pathways and summation of activities. Summation of the vertical sort ("peaking") is relatively more prominent in the fovea and associated systems while lateral summation is more prominent in the periphery.

Accurate screen-plate reproduction of retinal events upon the cortex is further broken up by *temporal dispersion*. Characteristics of the electroretinogram (*cf.* 3) indicate that even within the optic tract the "retinal image" is already somewhat dispersed in time. As successive synapses are traversed, further dispersion occurs; Bartley and Bishop (4, 5) have shown that a single brief flash produces a multiple response in the higher centers, extending over appreciable portions of a second. Closed circuits among interneurons at various levels can add to the dispersion of the image through time (26). The reciprocal overlap described above produces *spatial dispersion* of the retinal image:

"... quantitatively the unit paths near central vision should now be conceived, not as lines, but as expanding cylinders whose ends bear a ratio of 1:10,000, and a cellular ratio of perhaps 1:100. These unit paths then are related at each synaptic level by reciprocal dendritic overlap of increasing extent. . . . We must conclude that there is one primary cortical locus for each foveal cone. But multiplication of path makes that locus a group . . . of cortical cells, which would all have nearly



equivalent connections to the retinal cone" (29, p. 135).

Corresponding retinal points for the two eyes project to the same cortical area.

The end of complication is not yet. Continuous *movements of the eyes* further enlarge the neural region excited by a fine line or contour. The naive assumption—convenient for the theorist—that constant and perfect fixation is maintained during experimental observations is mechanically impossible for the eye-muscle system (*cf.* 29, pp. 136–139). Between 10 and 100 times per second there are tremors falling within 2' of arc (4 cone width), about 5 times per second fluctuations within 4' occur (8 cones), and about once per second there may be movements as gross as 30' (60 cones)—these movements are referred to as *physiological nystagmus*. Rather than being an imperfection in the receptive apparatus, it is this very "flutter" of the ocular system that makes possible the resolution of fine contours and, as a matter of fact, continuous vision at all in an adapting, fatigable system. Unless the visual field is perfectly homogeneous (a rare situation), this means that there are continuous changes in excitation, especially for cells near the borders of intensity differentials in the field. Under normal conditions, then, the "retinal image" itself is a shifting pattern of intensity gradients.

This revised picture of the projection system requires that a *statistical conception* be substituted for the classic geometrical one. Marshall and Talbot hypothesize that the projection of a fine line, or intensity contour, will be a "Gaussian distribution of connections symmetrical about its axis." Rather than obscuring analysis of visual functions, it now becomes possible to demonstrate how details and regularities, never feasible with the geometrical model, can be obtained. It is well

known, for example, that very fine "hair lines," as small as 1/60th the diameter of a single cone, can be discriminated, provided their length covers about 150 cones and they are projected on a bright, uniform background. How is this possible? According to Marshall and Talbot:

"The neural 'image' plays continuously over the projection area at every synaptic level, building gradients and peaks of activation at every edge and line. . . . Multiplication of path both increases the reciprocal overlap and refines the mosaic in proportion to the sharper gradients and peaks produced, as sand forms sharper peaks than bricks. . . . A fine line oscillating over 4 or 5 rows of receptors . . . [produces] a center of gravity of excitation which is further peaked at the center through the action of partially shifted overlapping connections" (29, p. 139).

#### APPLICATION OF THE STATISTICAL HYPOTHESIS TO FIGURAL AFTER-EFFECTS

We shall refer to the Marshall and Talbot type of analysis as the "statistical hypothesis." The proposed interpretation of figural after-effects is based upon their work, but requires certain additional assumptions, all of which seem to fall well within the framework of contemporary neurophysiological knowledge. Drawing directly on Marshall and Talbot, we assume (1) *that the representation of a contour in the projection cortex (area 17) is a normal distribution of excitation, symmetrical about its axis transversely and extending as a "ridge" throughout the longitudinal extent of the contour.* As discussed above, this distribution of excitation is produced by the simultaneous action of physiological nystagmus and reciprocal overlap of dendritic processes, and the distribution will be more or less "peaked" as a function of vertical summation.

The question immediately arises as to what types of fibers, and their central processes, are chiefly responsible for form and contour vision, i.e., what fibers contribute to this distribution representing a contour. We shall assume (2) that "on-off type" fibers and their central connections are chiefly responsible for the distributions of excitation in area 17 which represent visual forms, lines and contours. Although Marshall and Talbot do not definitely make this identification, it seems to be implicit to their analysis. At one point, for example, they say: "The fibers identified as carrying a regular succession of discharges during continuous photic stimulation . . . may serve an essentially protopathic function. Such a mechanism would serve to evaluate brilliance over larger areas, while the epicritic system would evaluate localized and fluctuating intensity changes. Both the neural systems would ride the photochemical adaptive curve, but presumably the protopathic would be a less independent function of adaptation" (28, p. 131). As a matter of fact, this interpretation seems required for their analysis, since they apply essentially the same considerations to black lines ("hair-lines") on white grounds and white lines ("bright bars") on black grounds. The assumption is necessary for interpretation of the figural after-effects for exactly the same reason. In other words, fluctuating change in the intensity of stimulation is what characterizes contours in the visual field, and the "on-off" mechanism is ideally designed to record continuously such events. It has been shown that "on-off" reactions are associated with receptor processes in the retina; further, these activities are extensively represented in the optic cortex and are quite probably fundamental to accurate form perception (5, 6, 7, 20).

A third assumption is (3) that the

rate of excitation of the mechanisms responsible for contour perception will vary directly with (a) their nearness to intensity gradients on the retina and (b) the sharpness of such intensity gradients. This follows directly from the fact that "on-off" receptors respond with a brief burst of impulses to changes in the intensity of stimulation (cf. 20), ceasing to fire under conditions of constant stimulation. This means that receptors beyond the range of the fluctuations produced by physiological nystagmus, whether continuously stimulated by the "ground" intensity or by the "figure" intensity of a large enough form or contour, will rapidly achieve a non-active state. On the other hand, the more often the intensity change representing the line or contour passes over an "on-off" receptor (i.e., the nearer its location with respect to the intensity gradient), the more frequently will it deliver bursts of impulses! Since these bursts of impulses are somewhat prolonged in time, fibers located near such fluctuating gradients will be in continuous or near-continuous activity, at rates determined again by their location. Following the well-known "intensity-frequency principle," the magnitude of reaction in "on-off" receptors will vary directly with the amount and rate of intensity change. Therefore, the amount of excitation per unit time in such mechanisms will also vary with the sharpness of the intensity gradient constituting the line or contour. This implies that, within certain limits dependent on irradiation locally within the retina and diffusely within the globe of the eye, contour resolution, figural after-effects and related phenomena will vary with the sharpness of intensity contrast between figure and ground (Köhler and Wallach generally worked under conditions of high contrast). The reader should note that, although we have dealt here with the

rate of excitation of retinal "on-off" mechanisms, these effects must be reflected equivalently in Brodmann's area 17 of the striate cortex.

Now we arrive at assumptions directly relevant to figural after-effects, e.g., the effects to be anticipated from prolonged inspection of a figure (system of intensity gradients). It can be assumed (4) *that under constant fixation of a figure, the cells in area 17 mediating the "on-off" activity will become differentially adapted as negatively accelerated functions of (a) the rate of their excitation and (b) the time through which they are excited.* In other words, the degree of adaptation to be expected from prolonged inspection of a contour is proportional to the degree to which given "on-off" processes are affected by that contour. The number of impulses which a sense organ discharges per sec. depends not only on the intensity of the stimulus, but also on the length of time through which the stimulus has been operating (1). Furthermore, the response of an excitable system is influenced by fatigue, a somewhat slower developing state than adaptation, dependent upon oxygen levels (10). Axones are subject to both types of phenomena (32).

However, it must also be assumed (5) *that such adaptation gradients will become flattened during recovery periods, since recovery from effects of previous adaptation is a negatively accelerated function of its degree.* The greater the rate of previous excitation, the faster the initial rate of recovery—and since excitation rate has been faster about the center of the distribution representing the inspection contour, recovery will occur at a faster rate here, thus flattening the adaptation distribution. It should be pointed out that the lateral extent of the effects dealt with here would be greater toward the pe-

riphery, and the Köhler and Wallach technique was such that observations were generally made well outside the foveal region.

A final assumption, also in agreement with Marshall and Talbot, must be made, namely, (6) *that the apparent localization of a contour in subjective visual space coincides with the location of maximal excitation in area 17.* In other words, the statistical distribution of excitations is not perceived as a graduated "blur," but rather as a fine point or line representing the locus of the fastest rate of excitation. The restriction of this postulate to area 17 is for convenience only. For our present analysis it is not immediately necessary to bring in larger areas of the cortex. It is recognized, however, that excitation in area 17 will rather directly influence areas 18, 19 and 20 with more far reaching consequent influences (*cf.* 9). It is also recognized that inputs from other areas into area 17 are important. It is probable, for example, that such phenomena as set and attention are in part of this order. Hebb (21) has offered an extended theoretical analysis of this point.

For present purposes it is merely necessary to show that apparent localization can be shifted predictably for "test" contours following prolonged observation of "inspection" contours. Figure 2 illustrates this shift in maximum excitation as following directly from the assumptions made above. Curve  $I_a$  represents the hypothetical distribution of adaptation in on-off processes in area 17 immediately following prolonged inspection of a contour,  $I$ . In the interval before the  $T$ -contour can be presented and fixated, differential recovery from fatigue flattens this distribution to curve  $I_b$ . When the  $T$ -contour falls objectively somewhat to one side or the other of the previously inspected  $I$ -

contour, the normal, bilaterally symmetrical distribution of excitation it would ordinarily produce, curve  $T_a$ , is modified by the differential excitability of this region to curve  $T_b$ . Since the apparent localization of a contour in visual space depends upon the locus of maximal excitation, the apparent localization of the T-contour must be shifted from T to T'. With appropriate pairs of T-figures (to make possible simultaneous comparison), this shift in apparent spatial localization of a contour may appear simply as that—*displacement*—or, if the I-contour had completely surrounded the T-contour, the *size* of the included figure will appear to shrink. And, if the observer is "set" to make distance judgments, the same size effect is interpretable as increased *distance* from the observer. Since contrast is a function of the total amplitude of excitation at the contour, and since total amplitude is reduced within the region of relative adaptation, the borders of the affected T-figure will appear *paler* than those of the comparison T-figure. These are the major characteristics of the figural after-effect, as reported by Köhler and Wallach.

#### ANALYSIS OF PARTICULAR CHARACTERISTICS OF FIGURAL AFTER-EFFECTS

(1) *The distance paradox.* From a superficial application of either the Köhler and Wallach theory or the statistical theory, it would follow that the magnitude of after-effects should increase regularly as the T-contour is made to lie closer and closer to the location of the previous I-contour. Within limits this is true. In the situation described in Fig. 3 (A), for example, the I-figure is always observed with the eyes fixated on the lowest X; when progressively higher X's are used with the T-figures (thus gradually increasing the distance between the left-hand T-square and the "satiated" area), the magnitude of the displacement decreases. But this is not the whole story. Following inspection of the angle shown in Fig. 3 (B) the *right-hand* pair of T-squares appears closer together than the left-hand pair, despite the fact that they are further from the previous contour. And in Fig. 3 (C) the right-hand pair of T-circles appears closer than the left-hand pair, following prolonged observation of the I-lines—again de-

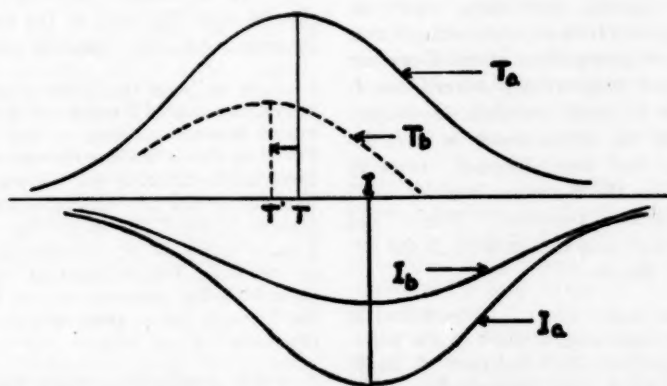


FIG. 2. Shift in apparent localization of a test contour following inspection of a neighboring inspection contour, according to the statistical theory.

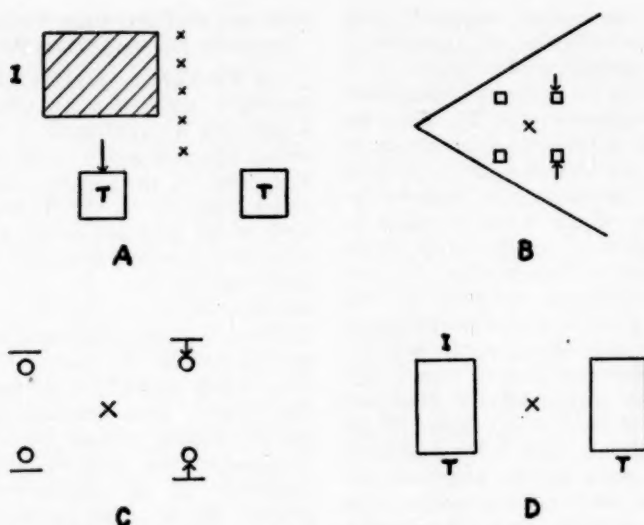


FIG. 3. Illustrations of displacement effects, after Köhler and Wallach (24): A, p. 275; B, p. 288; C, p. 289; D, p. 271. (See text for discussion.)

spite the greater distance. Finally, as shown in Fig. 3 (D), if the contour of the T-figure precisely coincides with that of the previous I-figure, no displacement occurs whatsoever—in the region where the greatest satiation has presumably occurred.

The general conclusion, based on some quantitative measurements, is that as the objective locus of the T-contour is moved progressively toward the I-contour by small amounts, the magnitude of the displacement at first increases and then decreases, reaching zero when the T-contour coincides with the previous I-contour. Köhler and Wallach explain this paradox in the following manner:

"Let us imagine that . . . (the T-line) is step by step brought closer to the I-line. As we proceed, more and more of the T-current which would normally flow in the direction of the affected area will now turn away . . . for, more and more, that cur-

rent meets with the increased resistance of the satiated area . . . therefore, the T-line will be displaced" (24, p. 337).

This applies up to the point where the displacement effect is maximal. Now, as to what happens in theory as the T-contour is brought yet nearer coincidence with the locus of the previous I-contour, i.e., the distance paradox:

" . . . as we bring the T-line nearer and nearer, the satiated I-region will gradually extend beyond the place of the T-line. The more this is the case the less will the T-current be deflected, and the less therefore will the line be displaced, because the position of the T-line within the satiated I-region begins to be progressively more symmetrical. This development will continue until the resistance on one side of the T-line is just as great as it is on the other side" (24, p. 338).

A stable equilibrium exists when T- and I-figures coincide in objective location.



Köhler and Wallach rightly consider this distance paradox a crucial point, and the Marshall and Talbot type of analysis must be able to account for it if it is to have any claim to adequacy. As a matter of fact, just such a prediction can be shown to follow directly from the previously stated assumptions. In Fig. 4, four degrees of approach of the T-contour to the I-contour are shown, along with complete coincidence. Only the somewhat flattened adaptation curve following a brief recovery period is drawn (i.e., in the Köhler and Wallach procedure a short interval intervenes between observation of the I-figure and fixation of the T-figure).<sup>1</sup> On the assumption that rate of fire in a differentially adapted region is reduced in direct proportion to degree of adaptation, this curve is simply subtracted directly from the symmetrical Gaussian distribution representing the excitation pattern that would have been produced by the T-contour. This leaves a non-symmetrical distribution (dashed curves in Fig. 4) of actual excitation whose point of maximal activity determines the apparent localization of the T-con-

<sup>1</sup> The adaptation curve was obtained in the following manner: According to statement No. 5 above, cells in the affected region of area 17 will recover from adaptation at rates dependent upon their previous degree of adaptation, but all following the same negatively accelerated function and all through the same period of time (e.g., that between removal of the I-figure and presentation of the T-figure). Using a single, logarithmic recovery function ( $y = a'$ ) and arbitrarily assuming that the peak of the adaptation distribution has been reduced to one-half of its initial height, one can graphically determine the "time" (distance along the horizontal axis) required for this amount of recovery. Using this time-distance as a constant, one can then determine how much relative recovery would occur at any point in the adaptation distribution by simply inserting this constant interval at that place on the recovery curve indicated by the initial altitude of the adaptation distribution at that point.

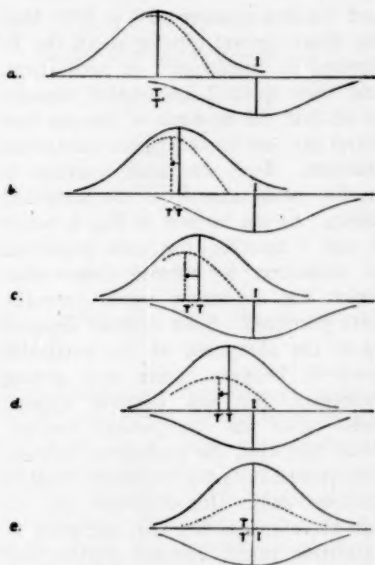


FIG. 4. Prediction of the "distance paradox" from assumptions of the statistical theory. (See text for discussion.)

tour. Obviously, if the central peak of the T-distribution falls beyond the range of I-adaptation (a), or if it precisely coincides with the distribution of I-adaptation (e), there can be no displacement in the locus of maximal excitation for the actual T-distribution. Between these two extremes, however (b, c, d), displacement of the locus of maximal excitation must first increase and then decrease as the T-contour is moved objectively closer to the previous I-contour. This follows from the form of the Gaussian distributions, and, as far as we have been able to determine, is independent of particular values which may be given these distributions.

Although there is no displacement when T coincides objectively with I, the T-contour does appear paler than a comparison contour (i.e., contrast with the ground is reduced). Köhler

and Wallach mention (24, p. 337) that the direct current flowing about the T-contour is "weakened" in such cases, and their general assumption appears to be that the strength of current flow about the contour of a figure determines contrast. Such decreased contrast is readily predictable from the statistical theory: As can be seen in Fig. 4, where T and I coincide, the total amplitude of excitation is reduced below that which the T-contour would normally have produced. Since contrast depends upon the sharpness of the excitation gradient between figure and ground processes, the test contour appears paler than the comparison contour. Note also that the excitation distribution representing the T-contour must be flattened under this condition, and one can therefore predict that apparent localization would fluctuate rapidly over a narrow region (as random variations change slightly the locus of maximal excitation rate); this would appear subjectively as "blurring" or indistinctness of the contour. Just such effects are readily verifiable under conditions of prolonged inspection.

(2) *Displacement effects of solid vs. outline I-figures.* Köhler and Wallach present data (24, p. 302) indicating that *the distance from an I-contour at which displacement of a subsequent T-contour is maximal is greater for outline I-figures than for solid I-figures.* Superficially, this statement is also paradoxical, for it would seem that a dense, solid object in the visual field should produce greater satiation of the medium. In their explanation of this fact, Köhler and Wallach indicate clearly that their theory, too, is concerned with *contour* effects rather than figural processes as such. The direct currents set up by contours are presumed to flow in every direction about the boundary of a figure, both within and without.

"With an oblong of very great width they are free to penetrate deeply into this area. . . . Within a narrower oblong, their spread around one boundary is therefore limited by the fact that the lines of flow which surround the opposite boundary claim half of the interior for *their* spread. . . . they are here forced to keep nearer the boundary than they would in a wider oblong. But this must have an effect upon those parts of the lines which pass through the environment of the oblong. . . . the pattern of flow will be pushed toward the outside when the oblong shrinks" (24, pp. 339-340).

Exactly the same differences between solid and outline figures would be expected on the basis of the statistical theory. As soon as the I-figure becomes so narrow that the continuous fixational eye-movements begin to *span* the retinal angle included by the figure, on-off mechanisms about the contours will begin to be affected by the changes in intensity characteristic of *both* edges. This will produce a deeper adaptation gradient close about each I-contour, thus further displacing the central tendency of the subsequent T-distribution than would be the case without such an overlap. Since the extent of lateral dispersion of impulses is greater in the periphery than near the fovea, due to the anatomical connections in this region, it can also be predicted that the area over which figural after-effects can be observed will be wider peripherally than parafoveally. Köhler and Wallach report observations on this matter, concluding that "satiation spreads wider in the periphery of the visual field than it does in parafoveal regions" (24, p. 303), but they fail to perceive this fact as being incompatible with their assumption of a homogeneous medium in which direct currents flow with disregard for anatomical connections.

(3) *Horizontal vs. vertical effects.* Most of the after-effect phenomena con-

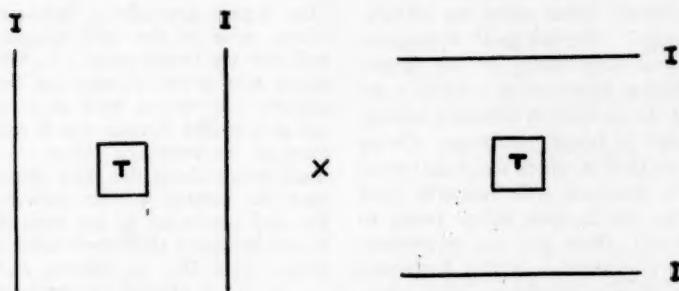


FIG. 5. Conditions for demonstrating horizontal vs. vertical after-effects, after Köhler and Wallach (24, p. 278).

sidered so far have been interpretable from either theory with equal ease. This is not true of the case which follows. Köhler and Wallach present the situation shown as Fig. 5 as one of their many demonstrations. The two T-squares are equidistant from the fixation point, but one is placed in a region previously surrounded by vertical I-lines while the other falls in a region previously surrounded by horizontal I-lines. The objective distance between T-squares and I-lines is the same in both cases.

"Nevertheless, their after-effects were found to be perceptibly different: the left square lay back in space. It seems to follow that the figure process between the vertical parallels is not quite the same as that between the horizontals, that it is more intense between the verticals. Our pattern represents a special instance of the so-called Vertical-Horizontal illusion. Visually the vertical lines are longer than the horizontal lines. For the same reason the horizontal distance between the vertical lines is visually shorter than the vertical distance between the horizontal lines" (24, p. 278).

This suggested explanation—that the vertical-horizontal illusion makes horizontal distances in the field subjectively shorter—certainly does not follow from their theory. As a matter of fact, ver-

tical extents are not always judged longer. Referring to the cross in the middle of Fig. 6, most naive observers (or sophisticated, for that matter) perceive no differences in the lengths of vertical or horizontal lines. The lefthand figure shows the comparison as it is usually given, and the vertical line is clearly longer subjectively. But in the righthand figure, the horizontal line definitely appears longer! Needless to say, all lines are objectively equal. The explanation may lie in differences between central and peripheral acuity: With fixation "naturally" falling at the points of intersection, the vertical line in the lefthand figure and the horizontal line in the righthand figure extend farther into the periphery.

This leaves unexplained the fact that horizontal after-effects are demonstrably greater than vertical after-effects, as far as the Köhler and Wallach theory



FIG. 6. Conditions for testing horizontal-vertical illusions: as usually presented (lefthand figure); in a way which makes the horizontal line seem longer (righthand figure); and in a way which eliminates the illusion (center figure). All lines are of equal objective length.

is concerned. What about the statistical theory? Physiological nystagmus contributes importantly to the spread of excitation representing a contour, by causing the contour to fluctuate rapidly over rows of retinal receptors. Owing to the manner in which the ocular muscles are attached and balanced (and probably also to some extent owing to experience), *these fine eye movements are more extensive in the horizontal than in the vertical plane.* This means that the distribution of excitation representing a vertical line in the field will be broader about its own axis than that representing a horizontal line. From the other assumptions we have made, it follows that figural after-effects will tend to be greater in the horizontal plane than in the vertical plane. Another observation made by the writers is pertinent at this point: If the intersection of a cross like the center figure in Fig. 6 is fixated for a period of time and at such a distance that the lines are near the limits of acuity, the *horizontal* line can be clearly seen to disappear from time to time while the vertical line remains constant. As Dodge long ago argued (13, p. 10), the constant application of a stimulus to a single receptor should result in its becoming invisible (through adaptation). In the present instance, because of the nature of eye movements, the horizontal line is restricted to a narrower range of elements than the vertical line and therefore, near the limits of acuity where the range of elements is smallest, it does repeatedly become invisible.

(4) *Figural after-effects "in the third dimension."* In a more recent communication by Köhler and Emery (23), the depth and distance effects only mentioned in the earlier monograph are more fully explored. On the basis of a number of demonstrations some of which we shall presently study, the following conclusions are drawn:

"The figural after-effects discovered by Gibson occur in the third dimension as well as in the frontal plane. . . . When an object at a certain distance has been inspected, test objects both at a greater and at a smaller distance recede from the place of the inspection object. . . . This displacement shows the same dependence upon the distance between inspection-object and test-object as has been demonstrated for figural after-effects in the frontal plane [specifically, the distance paradox]. . . . So far as after-effects are concerned, the third dimension must be measured with reference to the plane of fixation or the horopter. . . . Figural after-effects in the third dimension are as such concentrated about contours; but less affected parts of surfaces assume shapes in the third dimension which fit the displacement of their contours. . . . From the existence of localized figural after-effects in the third dimension, it is concluded that visual depth is a sensory fact" (23, p. 201).

Although these authors claim that third dimensional effects cannot be explained in terms of after-effects in the frontal plane, i.e.; on a two-dimensional basis, it will be our suggestion that the phenomena described can be attributed to the same contour displacements or size changes discussed above. It is well known that changes in size or distance are reciprocal interpretations when cues are ambiguous, and when observers are set for the latter (as was pretty clearly the case in the Köhler and Emery study) changes in size may be judged as changes in distance.

As their first demonstration of third dimensional after-effects, Köhler and Emery deal with tilted lines. A horizontal I-line is presented to S in such a manner that there is a 20° displacement from the frontal plane (see Fig. 7). After prolonged observation, the gaze is shifted to another line which is normal to the frontal plane, and this line now appears tilted in the opposite direction. In Fig. 7 (B) a graphic explanation of this effect in terms of ordi-

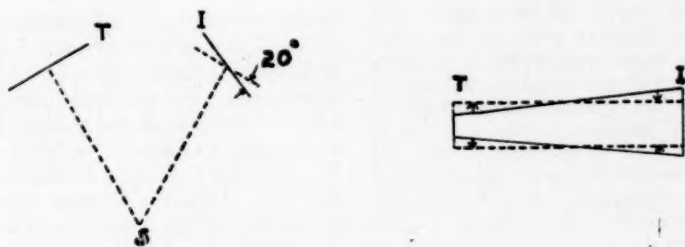


FIG. 7. A, conditions for demonstrating the tilting of lines in the third dimension, after Köhler and Emery (23, p. 162); B, interpretation in terms of simple contour displacements. (See text for discussion.)

nary displacements of contours is shown. The fact that the I-line is not normal to the frontal plane must mean that the *size* (e.g., visual angle subtended on the retina) of the nearer portion is greater than the size of the farther portion. When the fixation is shifted to the normal T-line, the portion corresponding to the nearer part of the I-line must fall *within* the contours of the previous I-line, and hence become smaller, while the portion corresponding to the farther part of the I-line must fall *outside* the contours of the previous I-line, and hence be pushed outward. This size effect is continuous throughout the extent of the T-line and is interpreted as a distance change, i.e., the T-line appears tilted in the opposite direction from the previous I-line. A more complicated case of the same sort is shown as Fig. 8. Since the entire I-card, with its vertical stripes, is concave toward the subject, the ends of the stripe will have a greater retinal size than the central portions. When the I-card is removed, revealing the normal vertical lines, the ends of these lines must again fall within the previous contours of the I-stripes while their centers do not. If the smoothly graduated decrease in apparent size toward both ends be interpreted as a depth effect, the normal T-lines will appear convex to the subject. It

is also reported that the depth effect is more striking with vertical lines than with horizontal lines, which—recalling the previous analysis of vertical vs. horizontal effects—is quite in keeping with the statistical theory.

Now we turn to relative spatial location or apparent distance. Although Köhler and Emery do not definitely defend the position, they seem to toy with the possibility that the third dimension in experience is founded upon an actual solid or “layered” topography of the cortex in which the figural processes can be “pushed” forward and backward with respect to one another. At one point, for example, they say:

“... few would support the notion that objects which appear at different distances from S are represented by processes on different levels within the cortex, some

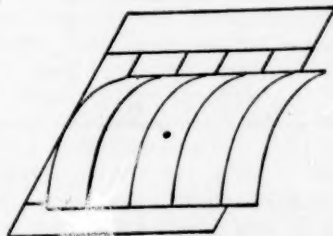


FIG. 8. A more complex situation for obtaining after-effects in the third dimension, after Köhler and Emery (23, p. 169).



nearer the surface and others lower. Yet, from the pragmatic point of view, there seems to be no serious harm in operating with a mental picture which pre-supposes precisely this topological representation of the third dimension in the visual brain. We have done so for quite a while, and have discarded the picture less because of certain observations which contradict it than because of its strangeness as a neurological idea. The fact that there are no immediate contradictions seems to prove that, to a large extent, the actual representation of the third dimension must be functionally isomorphic with the one which would follow from that picture" (23, p. 176).

The present writers do not fully understand the last sentence in this quotation, but, in any case, if it can be shown that third dimensional effects can be explained without postulating three dimensional processes in the visual brain, such conceptions become superfluous.

In the situation diagrammed in Fig. 9 (A), the solid lines represent white squares of identical size which are viewed against a large black screen. Following prolonged fixation of a point

on the same plane as the I-square, either  $T_1$  and  $T_2$  are substituted (nearer to S) or  $T_3$  and  $T_4$  (farther from S). In the former case the righthand T-square ( $T_1$ ) seems closer to the observer than its companion, while in the latter case the righthand T-square ( $T_3$ ) seems farther away from the observer than its companion. This is interpreted as an after-effect in the third dimension, which it certainly is in a phenomenal sense. However, keeping in mind that size and distance are interdependent interpretations—and assuming that the observers in this case were set for distance judgments—this effect is readily explained as another case of contour displacement. Since the inspection square and  $T_1$  are objectively equal in size, the fact that  $T_1$  is placed nearer in space than the I-square means that its contours must fall *outside* the location of the previous I-contours on the retina. Therefore, the displacement effects predictable from the statistical theory must be such as to expand the apparent size of  $T_1$  with respect to  $T_2$ , yielding the impression of being nearer.

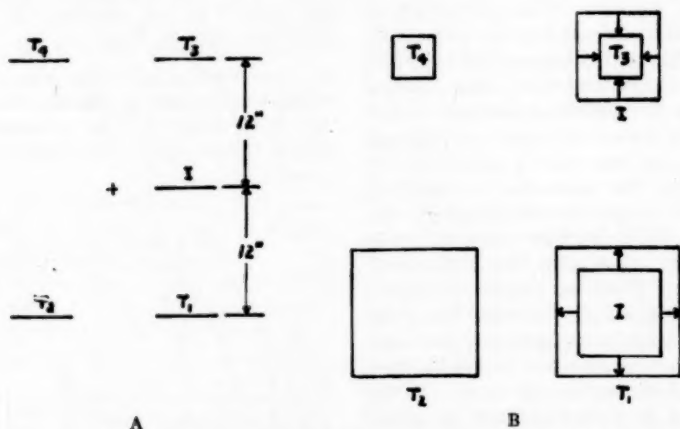


FIG. 9. A, conditions for demonstrating displacements in apparent relative distance, after Köhler and Emery (23, p. 178); B, interpretation in terms of simple contour displacements. (See text for discussion.)

By similar reasoning,  $T_2$  must fall *within* the contours of the previous I-square, will shrink in apparent size with respect to  $T_1$  and hence will yield the impression of being farther away. Diagrammatic presentation of this interpretation is given in Fig. 9 (B). Köhler and Emery also give quantitative data to show that the "distance paradox" characteristic of two dimensional after-effects applies here as well.

It might be expected that Köhler and Emery would interpret these depth and distance phenomena as has been done here—that is, as contour displacements in two dimensions yielding size changes which are interpreted as distance changes, since such an interpretation would fit the original Köhler and Wallach theory as well as the statistical one. But this is not the case:

"... the displacements in the third dimension cannot be interpreted in this fashion. A simple experiment which excludes the explanation is as follows. The I-object, which is placed at the distance of maximal displacement behind the T-object, is given a considerably larger size so that, in spite of its greater distance, its edges *surround* the T-object in retinal projection. Although under these conditions the two-dimensional after-effect makes the T-object shrink, this object is clearly displaced forward. In view of this fact, the explanation is not tenable" (23, p. 194).

Despite the crucial character of this test, the above is the only mention made of such observations. Since this represented the *only* negative instance (with respect to the Marshall and Talbot type of explanation) in the many demonstrations reported by Köhler and his associates, the present writers were understandably dubious as to its validity—especially since judgments about after-effects are influenced by a multitude of obscure factors and are difficult when the comparisons to be made

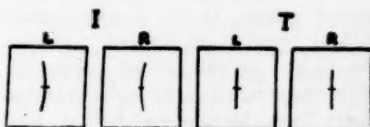


FIG. 10. Conditions for demonstrating third dimensional after-effects from stereoscopically fused images, after Köhler and Emery (23, p. 172).

are somewhat removed from the fixation point.

We have duplicated Köhler and Emery's procedure here in all essential respects, but have been unable to substantiate the apparent shift forward in the third dimension. Using ourselves as subjects (both more or less sophisticated and certainly biased), the only clear effect was a reduction in size of the righthand T-square. Using naive observers with respect to this problem, a size change was usually reported—and when a change in distance was noted, it was generally *farther away*. It will be fortunate if subsequent quantitative tests turn out this way. Even from Köhler's theory, a movement *forward* of an object made both smaller and paler than its neighbor presents a puzzling disregard of the cues normally utilized in distance judgments.

Köhler and Emery also report that tilted line effects can be obtained stereoscopically. They describe the situation shown in Fig. 10. After stereoscopic inspection of the curved lines (which, when fused, appear *concave* with respect to the observer), the the straight T-lines appear bent in the opposite spatial direction (*convex* with respect to the observer). *Neither the statistical theory nor Köhler's field theory can handle this phenomenon.* With central fixation on each of the monocularly presented curved lines, the images must fall on non-homonymous halves of the two retinae (here, with

crossed images, to the *temporal* sides of both vertical meridians). Projection in the primate visual system involves decussation at the optic chiasma. Fibers from the temporal half of each retina remain on the same side and terminate in the ipsilateral hemisphere, while fibers from the nasal half cross at the chiasma and terminate in the contralateral hemisphere (thereby providing that corresponding retinal points project to identical cortical points). This means that, in the present instance, the cortical representations of the two monocular images must be located in opposite hemispheres.

Köhler's field conception requires isomorphic relations between periphery and brain such that proximities in the visual field are paralleled by equivalent proximities in the brain field. Such isomorphism is feasible in area 17, given the nature of the projection system, but firing of impulses *from* area 17 into bordering area 18 (apparently the *only* transcortical connections for area 17) is diffuse, no point-for-point ordering being observable (9). If there were dense inter-hemispheric connections between contralateral areas 17, this might not be so serious, but considerable anatomical evidence (see below) makes this very doubtful. How, then, can direct currents flow about "figures" that are partly in one hemisphere and partly in the other? The statistical theory is no better off: The distributions of excitation upon which it depends would have to exist partly in one hemisphere and partly in the other. Nor can we deal with each half-image separately, as simple curved line displacements (e.g., Gibson's original phenomenon)—the monocular "displacements" in this case would have to be *across* the vertical meridian of the visual field, hence across the space between the hemispheres!

Anatomical evidence indicates that

fibers from the two halves of each retina, including the two halves of the fovea, go to opposite hemispheres. That the dividing line follows the vertical meridian is indicated experimentally in the monkey by cutting one optic tract beyond the chiasma and studying subsequent retrograde degeneration of the ganglion cells in either retina—degeneration is quite sharply limited to the homonymous halves of both retinæ (30, p. 438). Furthermore, when action potentials are recorded at the striate cortex while restricted regions of the retina are stimulated with light (34) and a "cortical map" obtained thereby, the hemifoveae and borders of the vertical meridian are shown to project to distinct and separate homologous regions of the two hemispheres. Both Polyak (30, p. 439) and Le Gros Clark (11, p. 227) draw the unequivocal conclusion that there is no bilateral cortical representation of the entire macular region of the retina. How can these spatially separated regions interact? Curtis (12), Bonin, Garol and McCulloch (9) and Le Gros Clark (11) all offer anatomical evidence that there are no neural pathways via the corpus callosum to integrate the two areas 17. There are numerous integrative possibilities between contralateral areas 18, but the diffuseness of transmission between 17 and 18 would not seem to permit precise binocular fusion.

All of this anatomical evidence adds up to a paradoxical state of affairs. The existence of the optic chiasma and the partial decussation that takes place there in animals with overlapping binocular fields is mute testimony to the elaborate provisions that have been made to insure that stimulation of corresponding points will be projected to the same cortical regions, thus providing for single vision. But the regions which provide the sharpest single vision functionally—the vertical merid-

ian and particularly the foveal centers—deliver their excitations to widely separated cortical loci. Yet, a fine vertical line, centrally fixated, is seen as single not double, despite the oscillations of physiological nystagmus; yet the phi-phenomenon can occur across the vertical meridian (15, 33); yet, all other transverse processes (contour formation, brightness and color summations and contrasts, depth effects and figural after-effects) seem to proceed across the vertical meridian as well as elsewhere in the field. In other words, the great anatomical gulf between the two halves of the visual field, established at the vertical meridian, does not appear in visual functions.<sup>2</sup>

What do neurophysiologists say about this? Marshall and Talbot (29, p. 132) admit that "more experiments are needed to reveal visual relationships affected by this division." Polyak merely states the paradox: "Dynamically, the entire primate visual system, essentially cyclopic in its character, is organized about the common binocular fixation-point. The same is true also of the cerebral eye, except that the single fixation-point is here split in two, one in each pole of the two occipital lobes, although even so, functionally, the two cerebral fixation-points may be regarded as a single point, always working as a unit" (30, p. 442). But how can these two anatomically separated fixation-points "work as a unit" if there are no anatomical provisions for

such teamwork? The wealth of *functional* evidence indicating close integration across the vertical meridian certainly suggests that some anatomical bases will be discovered. One possibility, not completely eliminated by existing evidence, is that some of the axones of optic nerves may *bisurcate* at the chiasma, to terminate in both lateral geniculates.<sup>3</sup> Presumably this would happen in proportion to the nearness of these processes to the vertical meridian (perhaps being drawn equally in both directions by gradients set up during embryological development). In any event, both statistical and field interpretations of after-effect phenomena flounder over this apparent gap in the projection system.

(5) *Temporal factors.* There is great need for more extensive quantitative data on figural after-effects. Köhler and his associates have generally been content with qualitative evidence, demonstrational rather than experimental in character. One quantitative investigation on temporal variables by Hammer (19) has recently been reported. Vertical lines served as both I and T figures, an angular distance of 2.2 mm. between them being kept constant. The affected T-line was 5 mm. below the fixation point and the comparison T-line was an equal distance above it. Displacements of the lower T-line, as a result of previous inspection of the I-line, were determined by the amount that the subject had to shift the upper T-line in order to make them fall on a single vertical plane. (a) *Magnitude of displacement was found to increase as a negatively accelerated function of the length of the inspection period, reaching an asymptote by about 60 seconds.* Such a function as this is typical of adaptation phenomena within sen-

<sup>2</sup> Since this writing a new book by Penfield and Rasmussen, *The Cerebral Cortex of Man*, has been published. Numerous cases are described in which stimulation of the bared cortex of conscious human subjects, with electrodes placed unilaterally in either area 17 or 18, resulted in reports of visual experiences localized in both halves of the visual field. While this also must be classed as further functional evidence, it certainly increases the likelihood that anatomical bases for interhemispheric interactions among visual systems will be uncovered.

<sup>3</sup> Suggested to us by Professor Verner Wulff, in the Department of Physiology at the University of Illinois.

sory systems. (b) With inspection-period constant (at 60 seconds), *magnitude of displacement was found to decrease as a negatively accelerated function of the interval between I and T presentations*, becoming unmeasurable after about 100 seconds. Just such a function would be predicted if rate of recovery from adaptation varies directly with its degree (*cf.* principle No. 5 above). While these results are those to be expected from the statistical theory, it is quite probable that they could also be shown to follow from Köhler's view—so this does not constitute a crucial test of theory.

(6) *Figural after-effects and Emmert's Law*. Prentice (31) has contributed an ingenious test which, since it proves inconclusive as reported, should be further studied. It is well known that ordinary after-images vary in apparent size with the perceived distance of the ground on which they are viewed. The after-image of a bright one-foot circle, originally viewed at a distance of 10 feet, appears like a small coin when viewed against one's palm but like a great balloon when projected against a far wall. This phenomenon, known as Emmert's Law, follows directly from the fact that the size of the visual angle on the retina subtended by the image is set by original inspection, and hence this differentially adapted region covers a greater or smaller area of the objective field depending upon the distance of fixation. The confused literature on *eidetic* imagery, on the other hand, suggests the possibility that these images do not follow Emmert's Law, their size being independent of known distance. Will figural after-effects follow Emmert's Law? According to the Marshall and Talbot type of interpretation they should, since figural after-effects are nothing more than the central results of projected "on-off" activity. What Köh-

ler's theory would say about the matter is not clear; if figural processes have some independent existence as wholes, it is possible that their after-effects would be independent of Emmert's Law. At any rate, Prentice reaches this conclusion.

The inspection and test figures used by Prentice are shown in Fig. 11. The test squares were affixed to the back of a piece of plate glass in a frame and could be independently raised or lowered. Either the Method of Constants (Experiment I) or the Method of Limits (Experiment II) was used to determine at what height the lefthand square appeared subjectively equal in height to the righthand square. The same subjects made judgments both without previous satiation (NS condition) and after previous satiation (S condition). In the first experiment, the I-pattern was always 2 m. distant from O while the T-pattern was either 2 m. or 6 m. distant (the observer simply rotating on a stool from I to T figures). In the second experiment (with the satiation area *below* the subsequent T-patterns), the inspection pattern was 3 m. from O while the T-squares were 3, 5 or 7 m. distant. In both experiments differences between NS and S conditions were significant, i.e., the after-effect due to previous satiation was obtained. In neither experiment, however, were differences in magnitude of the effect as a function of distance of

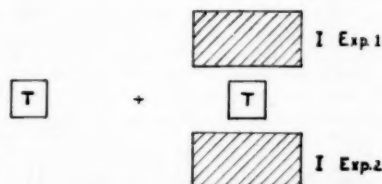


FIG. 11. Set-up for testing the applicability of Emmert's Law to after-effect phenomena, adapted from description in Prentice (31).



T-squares from the observer significant, i.e., measured displacement was independent of absolute distance. Prentice concludes:

"This study has measured the size of the Köhler-Wallach effect with distance varying and has shown that apparent rather than angular size determines the size of the 'satiated' area. These results lend themselves to the hypothesis that visual size is centrally determined as a result of interactions among various retinal and other stimuli, but that the entire complex behaves like a unit and is satiated as a unit" (31, p. 623).

There are at least two reasons why this conclusion must be questioned. (1) Prentice takes no account of the *distance paradox*, the fact that the magnitude of displacement has been shown to increase first and then to decrease as a function of the absolute distance between I and T contours. Now, we note that in all cases the T-squares were varied so as to be *farther* than the distance of the I-figure. This must mean that the absolute distance between I and T contours becomes larger as the distance from O becomes greater (i.e., the T-contour nearest the satiated area comes closer and closer to the medial plane). If, as was quite possible under Prentice's conditions, the absolute distance between I and T contours was always beyond the point of maximal effect, the decreasing after-effect would neatly balance off the enlargement to be expected from Emmert's Law. In any case, the complex function that displacement bears to absolute distance between I and T contours severely complicates interpretation of the data. (2) No account is taken of *physiological nystagmus* during supposedly constant fixation. Keeping in mind that these eye movements are roughly constant in average magnitude regardless of the distance of the point fixated, it follows that the *area* of the objective field cov-

ered by the movements will increase as the field is moved farther from the eyes. This being the case, the farther the T-squares (and their fixation mark) are from the eyes, the more variable in location will be the actual region of satiation with respect to the T-objects, and this should also be expected to increase variability in judgments.

### CONCLUSION

This paper has described a statistical theory of the functioning of the visual projection system along the lines developed by Marshall and Talbot (29), and has applied this theory to the various figural after-effect phenomena reported by Köhler and Wallach (24) and others. As far as we can determine, all of the phenomena accounted for by Köhler's field theory are equally well covered by the statistical theory. This does not constitute a disproof of Köhler's position. With the possible exception of horizontal vs. vertical effects, no facts have been submitted here which contradict his assumptions. On the other hand, whereas Köhler's theory requires the postulation of novel, non-neural electrical currents in the brain, the statistical theory is based upon accepted neurophysiological principles concerning a nervous system composed of single neurones with precise connections. Much research remains to be done by neurophysiologists and psychologists alike which, we feel, will clarify the physiological substrate of perception.

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# RELIABILITY, AMBIGUITY AND CONTENT ANALYSIS

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On the current psychological scene there is an omnipresent situation that seems to plague social and clinical psychologists. Sooner or later in the course of their empirical investigations they are forced to use a set of judges to classify some qualitative material. The material to be categorized is usually either responses to some particular stimulus context or else the context itself. This whole area of investigation has been dealt with in communications research and projective test analysis under the name of content analysis. Content analysis may be defined more generally as "a research technique for the objective, systematic and quantitative description of human behavior, particularly linguistic." See (1) for similar definition. As defined in this way content analysis applies to a large class of qualitative overt behavior: behavior of individuals in group situations, overt behavior in individual situations, the content of communications, responses from projective tests, content of motion pictures, protocols from group discussions, etc. In short, almost any time a psychologist wishes to describe or use a dependent or independent variable that he cannot measure with an objective instrument, he is forced to use pooled judgments.

This area is replete with problems, primarily because the data it is dealing with are very inexact and open to a multitude of interpretations. These interpretations in turn mainly depend on a sound personality theory. There are other problems involved in this technique, however, which do not depend upon such a theory. These

have to do with the traditional considerations of reliability.

## RELIABILITY

### *The Problem*

The problem may be stated as follows: Consider one dichotomous (pro-con) category. How are we to decide when a given percentage agreement among judges in their judgment of a population of items with respect to this category is high enough for the category to be a usable one? Obviously if the category cannot be defined *extensively* in a reliable manner it cannot very well be used to say anything about the items being judged. Hence there is a need for some statistic that will indicate when there is enough agreement for a category to be usable. How is such a measure to be developed?

In order to derive a reasonable statistic, let us analyze the judging situation. We shall deal always with the case of a dichotomous category. Discussion of reasons for this choice is contained in a forthcoming article (3). By this is meant a category for which two exhaustive possibilities are given (pro-con). The advantages of this type of categorizing over the more usual are: (1) *usually* psychologically easier to attend to one decision at a time, (2) assures logicity of choices given judge, (3) easier to analyze judgments.

Our typical judging situation is one in which the judges are confronted with a population of items that are to be judged with respect to a category (pro or con). A category is a set of criteria which define a certain property, class or relationship. Thus, the

proposition the judge must consider for each item is, "This item X has the property C." He then examines the evidence for this proposition and judges whether the proposition is true or false.

What we are interested in finding is whether the criteria are sufficiently well constructed and communicable enough to allow a group of judges to agree on an extensive definition of the category, that is, to agree on a definition of the category that is constructed by indicating which items belong in and which items belong out. If judges can agree sufficiently on this definition, we can use the category as a workable variable. If we cannot obtain sufficient agreement, we are not justified in using the category to make any statements about the material being categorized. The remaining question is, "What is meant by *sufficient* agreement?"

Let us consider exactly what we are seeking. It hardly seems enough to say that the judges' agreement is significantly greater than chance. We know they are not flipping coins (or the equivalent) because they were given criteria for categorizing. Almost *any* criteria would allow a better than chance agreement.

It seems that for this type of work we need a more stringent measure. What we should like to know is how many of the judges interpret the criteria in such a way that they agree on where the items belong. Perhaps we could take an arbitrary level and state that when, say, 90 per cent of the judges agree on the classification of all of the items, we can accept the category. This type of statement seems to indicate better what we are looking for in a reliable category.

How are we going to make sure that the 90 per cent agreement, or whatever level we choose, results entirely (within certain probability limits)

from an agreement on the part of the judges on the interpretation of the criterion? Chance will give us a certain level of agreement even if there is no congruence of judges' interpretations. We could not just take an empirical value of 90 per cent agreement as our acceptance point because part of that 90 per cent could be due to chance. Fortunately, elementary considerations from the calculus of probability allow us to calculate the appropriate percentages.

### Method of Scoring

First let us decide on a method of scoring judgments. To compute percentage agreement for an item, first choose one of the two possible decisions as "correct." This choice may be made by taking the consensus of all judges, or by taking one judge's decisions, or by any other method that is desired.

Then find what proportion of the judges agree with that decision. Add these proportions from all items and compute a final percentage agreement for the category.

Item	No. judges agreeing with "correct" category	Total judgments made
1	$S_1$	$t_1$
2	$S_2$	$t_2$
3	$S_3$	$t_3$
.	.	.
.	.	.
.	.	.
"	$S_n$	$t_n$
	$\sum_{i=1}^n S_i$	$\sum_{k=1}^n t_k$
		$\sum_{i=1}^n S_i$

$$\text{Per cent agreement for category} = \frac{\sum_{i=1}^n S_i}{\sum_{i=1}^n t_i} \times 100$$

### A Measure of Reliability

Before beginning the actual derivation, let us make explicit the assumptions being made:

(Assumption 1) Judgment is made between two exhaustive possibilities (a dichotomous decision). This actually amounts to classifying an item either in one category or not in that category (pro-con).

(Assumption 2) The population of items being classified is representative along the relevant dimensions of the population to which the results of the analysis are to be generalized. This would usually require a minimum of about 30 items.

(Assumption 3) The judges are all from the same population of competence. Competence refers to the knowledge the judge has of the evidence for the series of propositions, "This item has that property."

Let us assume that every judgment is made up entirely of  $U$  per cent based on the wholly correct interpretation of the criterion, and  $V$  per cent based on pure chance. Thus we can represent what may be called "sureness of a judge in his hypothesis." Suppose that a set of judges from the same population of competence judges one item the same way 80 per cent of the time. Their collective judgment may be represented as:

$$U\% \text{ Criterion} + V\% \text{ chance} = 0.8$$

If all of the judgments were made according to the correct interpretation of the criterion ( $C$ ) the judgments would have been all the same (i.e., the probability of selecting the "correct" category using the criterion = 1.0). If all judgments were made according to chance or random factors ( $R$ ) half of the judgments would be different from the other half (i.e., the probability of selecting the "correct" category using chance alone = 0.5). Thus we may write:

$$U \cdot 1.0 + V \cdot 0.5 = 0.8$$

by definition:

$$\begin{aligned} U + V &= 1.0; \quad V = 1.0 - U \\ \therefore U \cdot 1.0 + (1.0 - U) \cdot 0.5 &= 0.8 \\ U &= 0.6 \\ V &= 0.4 \end{aligned}$$

Thus we say that the judges' collective decision is based 0.6 on judges using criterion and 0.4 on chance factors. It is important to stress that this does not mean that the judges used a criterion 0.6 of their decision and flipped a coin, or used an equivalent method, for the other 0.4. It is merely a schematic way of representing a judgment.

We are now prepared to employ probability considerations. The judging situation in which one judge judges one item may be represented in the following way (for notation and derivation see Reichenbach [2]):

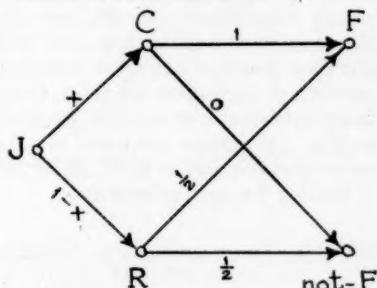


FIG. 1

$J$  = judge judges one item into one of two categories

$C$  = judge judges according to criterion (as discussed above)

$R$  = judge judges item randomly, according to chance (as discussed above)

$F$  = judge puts item into "correct" category, (i.e., the one called for by criterion)

$A \xrightarrow{p} B$  = probability from  $A$  to  $B = p$

We may now compute the inverse probability  $P(J, F, C)$ . This prob-



ability is read: The probability that if a judge agrees with the "correct" categorization then he is using a criterion (in the special sense of *criterion* as discussed above). This probability will tell us with what certainty we can assume that all of the judges are using the category criteria exclusively for their judgments, and that they are not using chance factors. More precisely the certainty refers to the use of a criterion which always gives the same result among judges. The certainty we wish to achieve is an arbitrary matter in the same sense that the choice of confidence levels is arbitrary. Which levels are most appropriate to use is a question that can only be determined empirically. We have found in the course of a few studies that the 10, 15, and 20 per cent levels were most reasonable. That is, those levels for which the value of  $P(J,F,C)$  equals respectively .90, .85, and .80. We are now in a position to solve this equation and find out what empirical percentage agreement we must obtain in order to be able to say that the probability that judges are using the criterion and not chance is .90, .85, or .80.

Solving for our probability:

$$\begin{aligned}
 P(J,F,C) &= \frac{P(J,C) \cdot P(C,F)}{P(J,C) \cdot P(C,F) + P(J,R) \cdot P(R,F)} \quad (\text{Rule of Bayes}) \\
 &= \frac{x \cdot 1}{(x \cdot 1) + (1 - x) \cdot \frac{1}{2}} = \frac{x}{\frac{1}{2}(x + 1)} = \frac{2x}{x + 1} \\
 P(J,F,C) &= \frac{2x}{x + 1} \quad (1)
 \end{aligned}$$

One more step is required to tie this expression to our empirical value of per cent agreement which we shall call  $A$ . Although the derivation now being made refers to only one judgment, we may treat the result (agreement or nonagreement with correct category) as a percentage for mathematical purposes.

The per cent agreement ( $A$ ) with the correct category is the same as the probability that the judge will put the item in  $F$  (see probability schema), i.e.,  $A = P(J,F)$ .

Hence:

$$P(J,F) = A$$

but:

$$\begin{aligned}
 P(J,F) &= P(J,C) \cdot P(C,F) \\
 &\quad + P(J,R) \cdot P(R,F) \\
 &\quad (\text{Rule of Elimination})
 \end{aligned}$$

$$\begin{aligned}
 P(J,F) &= x \cdot 1 + (1 - x) \cdot \frac{1}{2} \\
 &= \frac{1}{2}(x + 1)
 \end{aligned}$$

$$\therefore A = \frac{1}{2}(x + 1)$$

$$\text{and } x = 2A - 1 \quad (2)$$

We now have an expression for  $x$ , the only unknown in our probability diagram, in terms of an empirical value. If we make the necessary substitutions in our expression for  $P(J,F,C)$ , we can obtain the empirical value necessary to say that the probability is .90 that judges are all

using the criteria given. Thus:

$$\begin{aligned}
 P(J,F,C) &= \frac{2x}{x + 1} = \frac{2(2A - 1)}{2A - 1 + 1} \\
 P(J,F,C) &= \frac{2(2A - 1)}{2A} \\
 P(J,F,C) &= \frac{2A - 1}{A} \quad (3)
 \end{aligned}$$

Before completing this derivation for

$P(J,F,C) = .90$  let us state how (3) is read: The probability (in terms of empirical per cent agreement) that the judges are all using a criterion that leads to the same result OR that  $N$  per cent ( $N$  equal to confidence level chosen) of the judges are using such a criterion. The reason one judge and many judges are used interchangeably in this discussion is that when certain assumptions are made (see page 122) it does not matter whether the case of one judge judging several times is considered or the case of several judges from the same population judging once. To continue the derivation:

$$P(J,F,C) = \frac{2A - 1}{A} = 2 - \frac{1}{A}$$

$$.90 = 2 - \frac{1}{A}$$

$$\frac{1}{A} = 2 - .90$$

$$A = \frac{1}{1.10} = .909 \quad (4)$$

Using the same procedure we may also solve for the other parametric values of .85 and .80.

$$\frac{1}{A} = 2 - .85$$

$$A = \frac{1}{1.15} = .870 \quad (5)$$

$$\frac{1}{A} = 2 - .80$$

$$A = \frac{1}{1.20} = .833 \quad (6)$$

#### Sampling Considerations

We now have a parametric value for the case of one judge and one item (i.e., one judgment). This must now be extended to cover multiple judgment situations, that is, judgments involving either more than one judge or more than one item. The only relevant considerations are those of

sampling. We have chosen to handle this in the following way: We shall consider the parametric values we obtain for necessary percentage agreement as the means of populations of percentage agreement figures. The problem is now to guarantee in some way that the percentage agreement obtained for a larger sample of judgments differs from this parametric mean only by amounts due to sampling. This requires a slight modification of the sampling technique ordinarily employed. We shall choose the .05 and .01 levels of confidence for this population of percentages and require that for any given number of judgments the per cent agreement obtained must be higher than 95 per cent or 99 per cent of the cases in the population. That is:

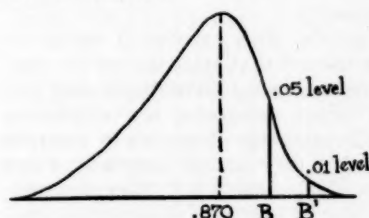


FIG. 2

Abscissa = population of per cent agreements for a given  $N$

Mean = .87 (for  $P(J,F,C) = .85$ )

Ordinate = frequency of occurrences of given per cent agreements ( $A$ ) in infinite number of samples of size  $N$ .

The requirement would be that the per cent agreement exceed the levels shown, that is to say, they must fall to the right of points shown ( $B$  for .05 level,  $B'$  for .01 level).

To accomplish this we simply make the inverse sine transformation (4) and use the Standard Error for that function which equals  $\sqrt{820.7/N}$ .  $N$  in this case is a number of judgments.

Thus for each  $N$  our minimal point of acceptance equals the parametric value plus 1.96 (or 2.58) times  $\sqrt{820.7/Nj}$ . These values are easily computed and are found in the tables following.

It may be argued that it is not reasonable to combine the agreements obtained for a population of items in one total figure because the easier items would compensate for the more difficult ones. However, we do not feel that this combining is undesirable since the purpose of categorization is to find and describe criteria well enough so that most of the items may be classified correctly with respect to those criteria. The statistic here described takes account of this objection and actually gives a criterion for the amount of compensation allowable.

At the other extreme it should be mentioned that statistics which combine agreement percentages from two or more categories are combining different kinds of sources of variance so that they cannot determine where the disagreement is. They are combining two different classes of propositions. Categories should be considered one at a time.

#### *Use of Tables*

To use these tables, follow these steps:

- (1) Determine the number of judgments (one judgment equals one judge judging one item) that are possible and practical to use for your content analysis.
- (2) Decide now (or at the end of your experiment) which agreement level is appropriate to your data—.90, .85 or .80.
- (3) Look up in appropriate table next to  $N$  = number of judgments chosen and find percentage

agreement figure required for acceptance of category at .01 level (top figure) or .05 level (bottom figure).

The general formula for computation of an agreement level  $A$  for  $Nj$  judgments at agreement level  $p$  (i.e.,  $P(J,F,C)$ ) for the .01 level of confidence is:

$$A = \frac{1}{2-p} + 2.58 \sqrt{\frac{820.7}{Nj}}$$

Let us take an example to clarify the use of these tables. An unpublished study by Gewirtz will serve. He has investigated children's behavior by selecting categories relevant to certain hypotheses and having trained observers categorize the behavior they observe. Examples of categories are nurturance-succorance, aggression-submission, etc. (The experiment has been altered slightly so as to serve as a clearer example.) The first step is to estimate how high an agreement level is appropriate for such data. Let us take the category of nurturance-succorance. To make a judgment for this category requires a considerable degree of skill. We shall therefore say offhand that if 80 per cent of the judges agree on all the behavior segments judged, we shall be satisfied that this category is usable. (More detailed consideration of the choice of agreement level is given below.) Hence we look at Table 3.

Next we decide that we can use three judges conveniently and judge about 50 items of behavior. Thus we enter Table 3 and look under 150 judgments ( $N$ ) and find that in order for our category of nurturance-succorance to be acceptable in the sense discussed in this paper, we must attain an empirical per cent agreement ( $A$ ) of 90 (.01 level of confidence) or 89 (.05 level of confidence).

If we do not reach this level empirically, we must refine our category criterion until we can achieve this level. When we do achieve it, we can be reasonably certain that 80 per cent of the judges are using the criterion in the same way, i.e., are categorizing in the same way. Or it might be stated

TABLE 1

AGREEMENT LEVEL  $[P(J,F,C)] = .90$  $N$  = number of judgments; $A$  = percentage agreement

$N$	$A$	$N$	$A$	$N$	$A$	$N$	$A$
2	100 100	35	99 98	100	97 96	260	95 94
3	100 100	40	99 98	110	97 96	280	95 94
4	100 100	45	99 97	120	97 95	300	95 94
5	100 100	50	99 97	130	96 95	350	95 94
6	100 100	55	98 97	140	96 95	400	94 94
7	100 100	60	98 97	150	96 95	450	94 93
8	100 100	65	98 97	160	96 95	500	94 93
9	100 100	70	98 97	170	96 95	600	94 93
10	100 100	75	98 96	180	96 95	700	94 93
15	100 100	80	97 96	190	96 95	800	93 93
20	100 99	85	97 96	200	95 95	900	93 93
25	100 99	90	97 96	220	95 94	1000	93 93
30	100 98	95	97 96	240	95 94		

TABLE 2

AGREEMENT LEVEL  $[P(J,F,C)] = .85$  $N$  = number of judgments; $A$  = percentage agreement

$N$	$A$	$N$	$A$	$N$	$A$	$N$	$A$
2	100 100	35	98 96	100	94 93	260	92 91
3	100 100	40	97 96	110	94 93	280	92 91
4	100 100	45	97 95	120	94 92	300	92 91
5	100 100	50	97 95	130	94 92	350	91 90
6	100 100	55	96 95	140	93 92	400	91 90
7	100 100	60	96 94	150	93 92	450	91 90
8	100 100	65	96 94	160	93 92	500	91 90
9	100 100	70	96 94	170	93 92	600	90 90
10	100 100	75	95 94	180	93 92	700	90 89
15	100 99	80	95 93	190	93 91	800	90 89
20	99 98	85	95 93	200	93 91	900	90 89
25	99 97	90	95 93	220	92 91	1000	90 89
30	98 96	95	95 93	240	92 91		

that 100 per cent of the judges are using the criterion the same way 80 per cent of the time.

We may now use the percentages obtained by our "correct" answers as being reliable, i.e., if 83 per cent of responses were classed as succorance this figure is usable for further considerations.

TABLE 3  
 AGREEMENT LEVEL  $[P(J,F,C)] = .80$

$N$  = number of judgments;

$A$  = percentage agreement

$N$	$A$	$N$	$A$	$N$	$A$	$N$	$A$
2	100 100	35	96 94	100	92 90	260	89 88
3	100 100	40	95 93	110	91 90	280	89 87
4	100 100	45	95 93	120	91 89	300	89 87
5	100 100	50	94 92	130	91 89	350	88 87
6	100 100	55	94 92	140	91 89	400	88 87
7	100 100	60	94 92	150	90 89	450	88 87
8	100 99	65	93 91	160	90 89	500	87 86
9	100 99	70	93 91	170	90 89	600	87 86
10	100 99	75	93 91	180	90 88	700	87 86
15	99 97	80	93 91	190	90 88	800	87 86
20	98 96	85	92 90	200	90 88	900	86 86
25	97 95	90	92 90	220	89 88	1000	86 86
30	97 94	95	92 90	240	89 88		

#### *Ambiguity and Agreement Levels*

We shall now discuss the concept of ambiguity. Let us, at this point, be careful to distinguish between two aspects of ambiguity. There is, first, the empirical question of how ambiguity arises in an individual perception. The answer to this question has to do with the intrinsic properties of the stimulus object, with the past

experience of the individual, etc. The other aspect, the one with which we are concerned, is the explication of the term, that is, what do we usually mean when we use the term "ambiguity." For our purpose we can list the usual meanings of ambiguity in the following way:

1. A stimulus object is said to be ambiguous if the structure of the stimulus is such that it resembles more than one thing with which we are acquainted (form ambiguity), e.g., Rorschach card.
2. A stimulus is said to be ambiguous if the stimulus situations portrayed could have two or more interpretations attributed to them (thematic ambiguity), e.g., TAT card.

Now let us take these two stimulus objects and put them in a different context. Let us suppose that an individual is confronted with a Rorschach card and is asked the question, "Is there any instance of the color red on this card?" In this situation the individual will not be confronted with an ambiguous situation, since the answer is a rather clear-cut one. In the case of the TAT card, if an individual is confronted with the card, and asked the question, "How many individuals are portrayed or represented on this card?," again the situation will not be an ambiguous one, since the answer again will be rather clear-cut.

This consideration leads us to question the proposition that ambiguity lies in the stimulus object itself. Our conclusion would seem to be that the ambiguity lies in the stimulus context, and the context consists of a stimulus object combined with a particular question asked of that stimulus object.

This result is important for content analysis for the following reason. It makes clear what we are interested in



investigating (*vis.*, ambiguity) when we wish to consider what agreement level we want to choose for a given content analysis. In other words, if a situation is more ambiguous, we are going to tolerate a much lower level of agreement than if the situation is relatively unambiguous. Hence, the fundamental question we must ask of every content analysis is what agreement level is the appropriate one. The question of whether we are to choose the agreement level before or after the content analysis has been made has been discussed elsewhere. However, the fundamental question that we are to ask is, "How ambiguous is this stimulus situation to the judge?" And by this question we are going to mean, "How much evidence is there for the question that is being asked of the particular stimulus material that is being considered?"

The next problem is how we are to establish the amount of evidence for a given question. At present I can see no precise ways of doing this. Considerations which should be taken into account would be the following:

1. A general, intuitive survey of the amount of evidence, including the amount of experimentation, that has been done in a particular area.
2. The results of other content analyses which have been done with roughly the same types of materials and roughly asking the same types of questions.

These are the primary scientific considerations. However, I believe a complete evaluation of the agreement levels acceptable is a contextual problem. That is to say, the agreement level that is acceptable is dependent on the purpose for which the content analysis categories or the agreement levels are going to be used.

For example, in a particular situation where the results of a content analysis are going to determine a large project of research, it would be appropriate to use a fairly stringent agreement level. On the other hand, if the results of an experiment are primarily heuristic, it is very possible that for the purposes of further experimentation, a much lower agreement level will be tolerated. In this respect, this is another instance of what occurs when confidence levels or limits are established. In this case, whether an investigator chooses the .05 or the .01 levels of confidence is dependent, again, as in content analysis, on the entire context, that is to say, on the purposes for which the results are to be used.

To summarize:

- a. The agreement level that we choose is dependent upon the ambiguity of the stimulus context.
- b. We measure ambiguity by the amount of evidence that is available for the question that we ask the judges. This question would always take the form, "Does item X have the property C?"
- c. We can find out about this amount of ambiguity by investigation of the amount of evidence, including the amount of experimentation that has been done towards answering this particular question, and also by investigating other content analyses that have used the same types of questions applied to the same types of material.
- d. We shall assume that, as more evidence is gathered, or rather, as more content analyses are made, more standardized agreement levels can be agreed upon, as in the case of confidence levels in classical statistics, where the same problem occurred. After much experimentation, using the same methodo-

logical framework, the .05 and the .01 levels, in general, were hit upon as being quite useful. It is true that in present-day statistics there has come to be some doubt as to the efficacy of using such rigid standards. There is a greater tendency to do the empirical work, compute the probability levels, and evaluate them in terms of the purpose to which the results are going to be put. I would suggest, at this early stage, that this type of approach is also appropriate to content analysis and reliability of judgments. As content analysis develops, the agreement levels will probably become more and more standardized.

#### *Discussion*

The uncritical way in which social and clinical psychologists handle the analysis of their qualitative data is perhaps one of the reasons why they receive so much criticism by the so-called "tough-minded experimental" psychologists. I feel that this particular criticism is somewhat justified. When a psychologist takes qualitative data and claims that they can be classified in certain ways, he is making a very important statement. Assuming the validity of the categories, he is saying that he has isolated some of the variables of behavior. To have accomplished such a task is a noteworthy event. If he can actually say that certain groups act in one way and other groups act in another, this is contributory. However, before he makes this claim the psychologist should be able to demonstrate that his classification system can withstand the scrutiny of statistical methods and the tests of reliability and repeatability. This demonstration has not been characteristic of studies in this area. Usually a certain percent-

age agreement among judges for a category (or sometimes a whole group of categories!) is ascertained, and some comment is made to the effect that "that's pretty good," or "we were satisfied." Rarely is there any statistical test given and more rarely is the statistical test appropriate to the data.

I feel that this attitude is not healthy. The discovery of variables through content analysis is an extremely important endeavor, especially in the present stage of psychology. As such it deserves the rigor that other areas have received. It is hoped that the measure presented herein will prove at least a start in this direction.

#### SUMMARY

1. The problem of judges' estimations of qualitative material occurs frequently in psychological research, especially in social and clinical psychology.

2. No satisfactory statistic has yet been devised to evaluate the percentage agreement among judges on rating an item with respect to certain categories.

3. The usual comparison-with-chance statistics are not adequate for this situation.

4. A statistic is introduced which gives the probability that the judges are all using the criterion given them for judging, and are not using chance factors. Several probability levels are given. The statistic essentially takes two factors into account: (a) the part of the empirical per cent agreement due to chance, (b) the total number of judgments made as a factor in reliability considerations.

5. Tables are presented for the statistic. A necessary-for-acceptance-of-category agreement level is given for almost all ordinarily used numbers of judges and items.

6. Choice of a probability level for a particular study depends on the ambiguity within the judging situation.

7. Ambiguity refers to the evidence for questions being asked, not to the stimulus material itself. The questions are of the type, "Does item X have the property C?"

8. Suggestions were given for rating ambiguity.

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## PERCEPTUAL ORGANIZATION IN THE RAT

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The course of modern research on the problem of discriminative learning has been markedly influenced by contradictory conceptions of the nature of perceptual organization. Lashley (10, 11) and Krechevsky (8, 9) have maintained that even in the rat perception is selective and relational in character, while Spence has insisted upon a purely additive organization, contending that discriminative behavior is a summative function of excitatory and inhibitory properties independently acquired by sensory components (15, 16). Although Spence's theory has been strongly supported by the results of experiments on continuity, transposition, and stimulus-generalization (1, 6, 17, 18), several recent investigations provide evidence for the operation of non-additive integrating mechanisms in perception. Saldanha and Bitterman (14) report that, under certain conditions at least, the progress of discriminative learning is facilitated by opportunity for direct comparison of the stimuli to be discriminated, a result which has been confirmed by Coate (3) in the context of a continuity experiment. Bosworth and Bitterman have found, conversely, that the relational introduction of *irrelevant* components *retards* discrimination (2). These results fit nicely into the conceptual framework developed by Lashley, but contradict deductions from the postulates of Spence. Neither theory, however, can deal with the results of an experiment by Weise and Bitterman (19) which suggested the greater fundamental simplicity of the successive as compared with the simultaneous type of discriminative problem. Taken to-

gether, these investigations suggest a distinction between two non-additive processes of perceptual organization—a primitive, diffuse situational process (configurational learning), and a more abstract, selective, transcontextual one (relational learning). The research to be reported here provides further support for the validity of this distinction.

The logic of the investigation can best be set forth in relation to the design of the experiment which is illustrated in Table I. Two rats, A and B, are trained in Lashley's jumping apparatus. The animals are matched for speed of learning Problem I, which is the same for both. When presented with two black-and-white vertically striped cards differing in width of stripe (Situation 1), the animals are rewarded for jumping left to the thin stripes and punished for going right to the thick stripes. The lateral position of these cards is never reversed, so that the thin stripes always appear at the left. When presented with two gray cards differing in brightness (Situation 2), the lighter card always being situated at the left, the animal is rewarded for jumping right to the dark card and punished for going left to the lighter card. The two pairs of cards are presented alternately in random fashion until the problem is mastered. Now training on Problem II is begun. This problem involves the same cards as the first, but the lateral inversions of the pairs employed in Problem I also are introduced, making a total of four different perceptual situations rather than only two as in the first problem. The situations are presented equally often in random sequence, and in this stage of the experi-

TABLE I  
ILLUSTRATION OF THE DESIGN OF  
EXPERIMENT II

Problem	Situation*	Rewarded response	
		Rat A	Rat B
I	1. thin/thick	left	left
	2. light/dark	right	right
II	1. thin/thick	left	right
	2. light/dark	right	left
	3. thick/thin	left	right
	4. dark/light	right	left

\* "Thin" and "thick" refer to black-and-white striped cards differing in width of stripe; "light" and "dark" refer to plain gray cards differing in brightness. "Thin/thick" means thin stripes in the left window of the jumping apparatus and thick stripes in the right window.

ment the two animals are trained differently. Rat A is required to jump left whenever the striped cards are presented, irrespective of lateral position, and to jump right when the gray cards appear, while Rat B is required to go right to the stripes and left to the grays. How will the performances of the two animals compare?

Spence's theory leads to the prediction that the animals will encounter equal difficulty. From an analysis of Problem I in terms of afferent components, we must conclude that it can only be mastered if thinness and darkness acquire dominantly excitatory properties while thickness and lightness acquire dominantly inhibitory properties; that is, the two thicknesses and the two brightnesses must be functionally differentiated. In Problem II, therefore, the animals should respond in terms of these differences in all four situations. The position reversals cannot from this point of view be significant, since leftness and rightness have been equally often rewarded in Problem I and any residual differences in

excitatory and inhibitory value must be negligible compared to the differences in the values of other components when the criterion of learning for the first problem has been reached. (In the experiment to be described residual position differential is randomized by the use of two *groups* of animals rather than two individuals.)

It follows, then, that the initial tendency of the two animals to jump to the thin stripes should be the same in Situations 1 and 3, and the initial tendency to jump to dark gray should be the same in Situations 2 and 4. Both learning curves, therefore, should begin at the chance (50 per cent) level, although the errors of Rat A should be made in Situations 3 and 4 while the errors of Rat B should be made in Situations 1 and 2. Furthermore, a strictly summative theory suggests that neither animal will ever master Problem II. If each afferent "component" is assigned an adience-value which is defined as the algebraic sum of its excitatory and inhibitory characteristics, the following inequalities must obtain at the time of mastery ( $P_1$  and  $P_2$  represent the values of the two positional components,  $T_1$  and  $T_2$  represent the two thicknesses, and  $G_1$  and  $G_2$  represent the two grays):<sup>1</sup>

$$T_1 + P_1 > T_2 + P_2$$

$$G_1 + P_2 > G_2 + P_1$$

$$T_2 + P_1 > T_1 + P_2$$

$$G_2 + P_2 > G_1 + P_1$$

Summing these inequalities, we arrive at the following invalid conclusion:

$$T_1 + T_2 + G_1 + G_2 + 2P_1 + 2P_2 > T_1 + T_2 + G_1 + G_2 + 2P_1 + 2P_2$$

A theory which assumes a purely additive relation among afferent components cannot, therefore, predict that errorless

<sup>1</sup> In the case of Rat A, Table I,  $T_1$  equals thin,  $T_2$  equals thick,  $P_1$  equals left,  $P_2$  equals right,  $G_1$  equals dark, and  $G_2$  equals light.



performance on Problem II is possible.

Despite the fact that Lashley's theory differs radically from that of Spence, it leads in this instance to much the same prediction. From the point of view of Lashley, in Problem I the animals have learned to choose the thinner stripe and darker gray, and this relational set should be manifested during the initial stages of training on Problem II. Once again, therefore, it must be deduced that both learning curves will begin at the 50 per cent level owing to the errors of Rat A in Situations 3 and 4 and the errors of Rat B in Situations 1 and 2. While Lashley's theory provides no basis for predicting that Problem II is impossible of solution, neither does it specify the processes which are involved in the mastery of such a problem. If selective, relational perception is fundamental to all discriminative learning, the solution of Problem II must require that an extremely complex pattern of conditional sets be developed. Although certain components of this pattern might conceivably appear during training on Problem I, it would seem more reasonable to assume that the animals do not resort to such elaborate "attempts at solution" (10, p. 243) in situations to which a completely satisfactory adjustment can be made on a more primitive level. If this assumption is correct—that is, if the animals achieved only uncomplicated relational preferences (for thinner and darker) in Problem I—they should progress at the same rate on Problem II.

A quite different prediction is suggested by the work of Weise and Bitterman (19). Although the experiments of Saldanha and Bitterman (14) demonstrate that stimulus-cards such as those employed in Problem I may, *under certain conditions*, be perceived relationally, the research of Weise and Bitterman indicates a second, more primitive configurational process which

takes precedence over the first when circumstances permit. From this point of view the two card-pairs (situations) of Problem I may function as diffuse or loosely organized wholes to which the animals learn to respond differentially (7). Since this primitive level of organization suffices for the solution of the problem, a more articulated organization, involving the differentiation of the two cards of each pair, will not readily be developed. If this formulation is correct—that is, if the animals learn in Problem I to go left to stripes (undifferentiated) and right to grays (also undifferentiated)—their performances on Problem II should differ significantly. If the cards of each pair remain completely undifferentiated at the termination of training on Problem I, the performance of Rat A on Problem II will be errorless, while Rat B may be expected to make many errors. In general, the difference in performance on Problem II provides an inverse index of the degree of differentiation developed in Problem I.

Before the results of this experiment are presented, a preliminary experiment which led to its design will be described. The general form of the preliminary investigation is schematized in Table II. Suppose that two animals are trained on a succession of three problems in Lashley's jumping apparatus. The animals are matched for performance on Problem I which is the same for both. This problem involves two situations, black/white and horizontal/vertical. In Situation 1 the animals are rewarded for going right to the white card and punished for going left to the black, while in Situation 2 they are rewarded for jumping left to the horizontally striped card and punished for jumping right to the vertical stripes. Problem II involves two situations, the previously encountered black/white and its lateral inversion,

TABLE II  
ILLUSTRATION OF THE DESIGN OF THE  
PRELIMINARY EXPERIMENT (I)

Problem	Situation*	Rewarded Response	
		Rat A	Rat B
I	1. black/white	right	right
	2. horizontal/vertical	left	left
II	1. black/white	right	left
	3. white/black	right	left
III	2. horizontal/vertical	left	right
	4. vertical/horizontal	left	right

\* "Black" and "white" refer to homogeneous black and white cards; "horizontal" and "vertical" refer to horizontally and vertically striped black-and-white cards. "Black/white" means that the black card was situated in the left window of the jumping apparatus and the white card was in the right window.

white/black. Rat A is rewarded for jumping right in both situations and Rat B for jumping to the left.

On the basis of the considerations already outlined, the theories of Spence and Lashley lead to the prediction that the two rats will master Problem II at the same rate. In Problem I both animals should acquire a preference for the white card which should be manifested in Problem II. The lateral position of this card should make no difference, since, in the language of Spence, each spatial component has been equally often rewarded in Problem I, and, in the language of Lashley, there is no reason for assuming that the spatial relation has been perceptually selected as relevant to the solution of Problem I. It follows from both theories, therefore, that the animals should respond initially at a chance level on Problem II (although the errors of Rat A will be made in Situation 3 while the errors of Rat B will be made in Situation 1), and that both animals should then proceed to a mastery of the problem at identical rates. Configurational theory

leads, on the other hand, to a quite different prediction. From this point of view the mastery of Problem I may involve, not a functional differentiation between black and white and between vertical and horizontal, but a more diffuse differentiation between the two perceptual situations as such. It is not implied that, following training on Problem I, Situation 3 will be fully equivalent to Situation 1, but only that the two situations are enough alike so that the animals will tend to respond to Situation 3 as they have learned to respond to Situation 1 (*situational generalization*). According to this theory, Rat A should have a considerable advantage while Rat B should be placed at a disadvantage, because in Situation 3 *both* animals should tend to jump right (to the black card) even though jumps to this card have previously been punished consistently.

As it turned out, however, differences in the performance of the rats on Problem II were not clear-cut, but only suggestive, and Problem III was introduced. Again on this problem the theories of Spence and Lashley predict no difference in rate of learning. Both animals must overcome preferences for the horizontally striped card established in Problem I and both animals must also overcome the position preferences established in Problem II. Configurational theory, on the other hand, suggests once more that Rat A will have a considerable advantage. Results for both problems will be presented following a detailed description of the procedure employed in the experiment.

#### EXPERIMENT I (PRELIMINARY)

*Subjects:* Twenty-two experimentally naive, male rats of the Wistar strain, ranging in age from 120 to 160 days, were employed in the experiment.

*Apparatus:* A two-window jumping apparatus, of the kind devised by Lash-

ley, was used. The windows were  $5\frac{1}{2}$  in. high and  $5\frac{1}{2}$  in. wide and separated by a wooden wedge  $1\frac{1}{4}$  in. wide which extended  $1\frac{3}{4}$  in. in front of the windows. The wedge served to discourage jumping to the strip between the two windows. The distance between the jumping platform and the windows was variable. The platform itself was covered with a grid through which weak shock could be administered to break resistance to jumping. A correct response admitted the animals to a feeding platform behind the windows, while an incorrect response precipitated the animals into a burlap net 3 ft. below. The stimulus cards appeared against a gray background.

*Procedure:* The major phases of the experiment were preceded by a period of preliminary training. The animals were fed on the feeding platform for several days and then allowed to walk through the open windows to food from the jumping platform which was moved up close to the windows. They were then trained to jump through gradually increasing distances, first to the open windows and then to unobstructed gray cards. Manual guidance was employed to ensure that the animals would jump equally often to both windows. Whatever position preferences were manifested during this stage of training were duly recorded. Throughout the experiment the rats were kept on a 24-hour feeding schedule.

*Problem I:* Following the preliminary training the animals were presented with Problem I, which is illustrated in Table II. Four stimulus-cards were employed, one black, one white, and two black-and-white striped cards, one horizontally and the other vertically, the width of each striation being  $\frac{1}{2}$  in. Each animal was trained to jump in one direction to one of the two possible spatial arrangements of the black-and-white cards (Situation 1) and in the op-

posite direction to one of the two possible spatial arrangements of the striped cards (Situation 2). There were thus eight different training combinations to which the animals were randomly assigned. Each rat was given ten trials per day, five to each of the two situations which were alternated following the Gellerman series (5). The non-correction method was employed throughout the experiment, and training was continued to a criterion of two errorless days. After this criterion was reached, each rat was given four days (40 trials) of over-learning.

*Problem II:* As the animals finished Problem I they were assigned to either of two groups, I and II, matched for rate of learning, and then trained on Problem II, which is illustrated in Table II. This problem involved the two situations black/white and white/black. Animals in Group I were trained to jump to both situations in the same direction as they did to the black-white pair in Problem I, while the animals of Group II were trained to jump in a direction opposite to that which was rewarded in the black-white situation of Problem I. For purposes of illustration it may be noted that Rats A and B of Table II belonged to Groups I and II, respectively. Again in this stage of the experiment, ten trials per day were given, the two situations being alternated randomly, and training was carried to a criterion of two errorless days.

*Problem III:* Following Problem II, each animal was trained on Problem III (see Table II). This problem involved the two situations horizontal/vertical and vertical/horizontal. In each of these situations, each animal was rewarded for jumping in a direction opposite to that which was rewarded in Problem II. Ten trials per day were given, five to each of the two situations which were alternated randomly, and

the criterion of learning was two error-less days.

### Results and Discussion

The course of learning for each group on each of the three problems is plotted in Fig. 1. The results for two animals of Group II which died during the early stages of Problem II were not used in computing these curves. The two groups learned Problem I at rates which are roughly comparable, the mean error score for Group I being 34.6 and that for Group II being 39.3. The over-all performances of the two groups on Problem II were also quite similar, the mean error scores being 6.5 and 8.3, respectively. The difference does not approach statistical significance, a result which can be deduced from the theories of Lashley and Spence. On the first day of Problem II, however, the scores of the two groups tended to diverge in a manner which cannot be predicted by these theories. On that day Group I made a mean of 2.35 errors while the mean error score of

Group II was 4.88. By Festinger's test (4) the difference is significant at about the five per cent level of confidence. Three animals of Group I actually made no errors at all on Problem II, and two made only one error each. No animal of Group II matched this performance. These results suggest that a real difference existed between the two groups which was masked, insofar as over-all performance on Problem II is concerned, by the extreme simplicity of that problem.

This interpretation is borne out by the results for Problem III. The animals of Group I learned rapidly while those of Group II showed a rigidly persistent tendency to jump in the direction rewarded in Problem II. The difference between the mean error scores of the two groups for the eight-day period of training (15.4 and 58.0, respectively) was significant beyond the one per cent level of confidence (Festinger's test).

The results of this preliminary experiment may be understood in the fol-

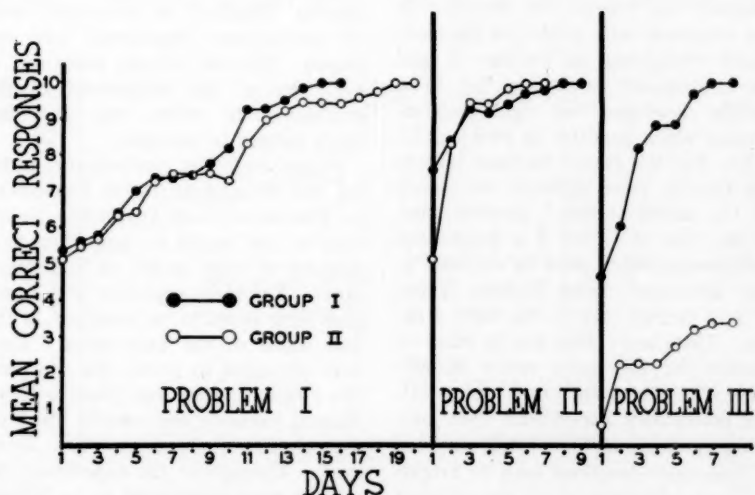


FIG. 1. Learning curves for the three problems of Experiment I.

lowing terms: In Problem I the animals learned to respond differentially to the two situations (black-white and horizontal-vertical) which were, to some extent at least, perceived as undifferentiated wholes. These configurations did not, however, remain totally undifferentiated, and there was some tendency for the positive and negative cards of each situation to become functionally distinct, as was revealed by the fact that some animals in each group continued for many trials on Problem II to jump to the positive brightness of Problem I irrespective of lateral position. The configurational effect was, nevertheless, manifested in a tendency toward *situational generalization* in Problem II (jumping in the previously rewarded direction to both black-white situations) which led to a significant difference between the first day's error scores. For Group I, situationally generalized responses were rewarded in Problem II, and for this reason the training on Problem II was partially congruent with that in Problem I, as was also the training on Problem III. For Group II, however, the training on Problem II was congruent with neither of the tendencies established in Problem I and the consequent frustration led to a rapidly developed but rigid position-fixation which persisted in Problem III (12). For this reason Problem III was significantly more difficult for Group II. It should be noted, however, that in the case of Group I a generalized position-preference must be assumed to have developed during Problem II and to have carried over to the third problem. There is no other way in which to account for the many errors initially made by these animals on Problem III. Our preliminary experiment, then, provides evidence for non-configurational or component-reactions both to brightness and to position of the sort required by current theories of discriminative

learning. Only if we make the assumption of an additional, configurational process, however, can we account for all of the results of this experiment.

## EXPERIMENT II

The second experiment was designed to demonstrate more clearly the configurational process revealed in the first. The conditions employed were chosen to minimize any tendency to respond in terms of particular aspects of the stimulus-situation which served to obscure the configurational response in the preliminary study. In the first place, new stimulus-cards were chosen with a view to making differentiation more difficult between the members of each pair. Further, Problems II and III of Experiment I were merged into a single second problem in order to forestall the development of obscuring positional reactions.

*Subjects:* Eighteen experimentally naive, male rats of the Wistar strain, ranging in age from 120 to 160 days, were studied.

*Apparatus:* The Lashley jumping apparatus described in connection with the preliminary experiment was employed. The only change made was in the color of the background of the stimulus-cards, which was now flat black instead of mid-gray.

*Procedure:* The preliminary training was the same as that in Experiment I. The animals were adapted to the apparatus and taught to jump through a distance of nine inches to unfastened cards. The cards employed for this purpose were those to be rewarded in the first phase of the experiment. They were presented as pairs—the two positive stripes and the two positive grays. Manual guidance was used to break up any position preferences which developed. Throughout the experiment the animals were maintained on a 24-hour feeding schedule.



**Problem I:** Following the preliminary training the animals were trained on Problem I, which is illustrated in Table I. The stimulus-cards were two homogeneous gray cards differing in brightness and two black-and-white, vertically striped cards differing in stripe-thickness ( $\frac{1}{4}$  in. and  $\frac{1}{2}$  in., respectively). For each animal only two pairings were used, the animals being required to jump in one direction to one of the two possible arrangements of the striped cards (Situation I) and in the opposite direction to one of the two possible arrangements of the gray cards (Situation II). As in Experiment I, therefore, there were eight different possible training combinations and to these the animals were randomly assigned. Each rat was given 12 trials per day, six with each of the two situations which were alternated according to modified Gellerman orders (5). The non-correction method was employed throughout the experiment and training was continued to a criterion of two errorless days.

**Problem II:** As the animals finished Problem I they were assigned to one

or the other of two groups, I and II, matched for rate of learning, and then trained on Problem II which is illustrated in Table I. This problem involved four situations, the two encountered previously in Problem I and their lateral reversals. Animals of Group I were trained to jump in the same direction as before to each of the old situations as well as to its lateral reversal, while animals of Group II were trained to jump in a direction opposite to that previously rewarded in each of the old situations as well as in its lateral reversal. Table I may be consulted for purposes of clarification. Rat A, which belonged to Group I, was trained in Problem I to jump left to one of the stripe-situations and right to one of the gray-situations. In Problem II it was required to jump left to both stripe-situations and right to both gray-situations. Rat B, which belonged to Group II, was trained in the same way on Problem I, but in Problem II was required to jump right to both stripe-situations and left to both gray-situations. In short, situational generalization from Problem I to Problem II was

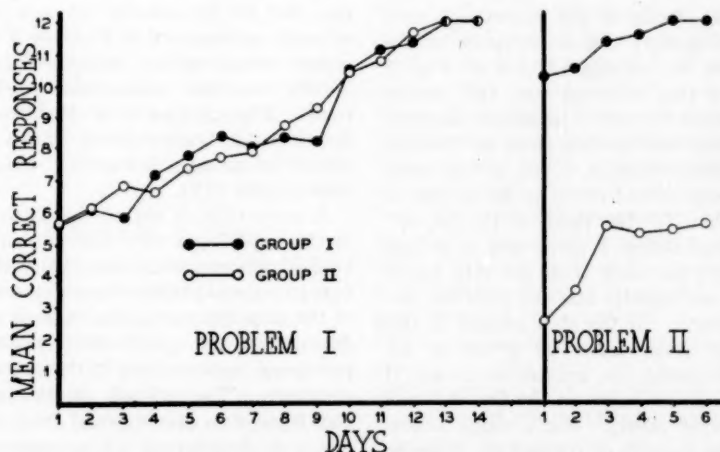


FIG. 2. Learning curves for the two problems of Experiment II.

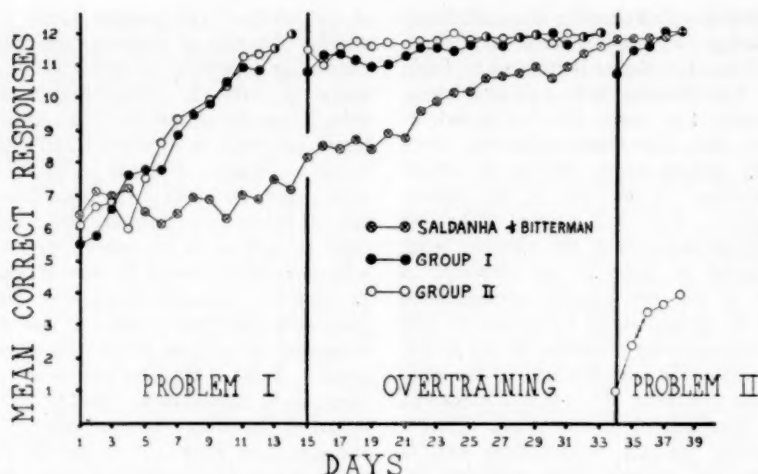


FIG. 3. Learning curves for the two problems of Experiment III and for the comparable four-situational problem of Saldanha and Bitterman (14).

rewarded for Group I and punished for Group II. Again in this stage of the experiment 12 trials per day were given, three to each of the four situations, and training was carried to a criterion of two errorless days.

#### Results and Discussion

The results of this experiment were unambiguous. An examination of the curves of learning plotted in Fig. 2 shows that although the two groups mastered Problem I in almost identical fashion, their performances on Problem II differed sharply. Both groups made a mean of 45.2 errors in the course of Problem I. On Problem II, the animals of Group I performed at a high level of accuracy from the very beginning and rapidly attained errorless performance. Of the nine animals in this group, three made no errors at all. Contrariwise, the animals of Group II made many errors on the first day and improved slowly. Not a single animal of this group had reached the criterion by the end of the sixth day of training,

at which time, all of the animals of Group I having mastered the problem, the experiment was terminated. During the six days of training Group I made a mean of 4.1 errors, the difference being significant at well beyond the one per cent level by Wilcoxon's test (20). It may be concluded, therefore, that *the two members of each pair of cards encountered in Problem I remained almost entirely undifferentiated despite consistent reward and punishment.* The card-pairs of Problem I functioned as unarticulated wholes to which the animals learned to respond differentially (19).

A comparison of the results obtained in this experiment with those obtained by Saldanha and Bitterman (14) with a four-situational problem involving cards of the same characteristics suggests the development of qualitatively distinct perceptual organizations in the two experiments. The animals of Saldanha and Bitterman were exposed from the outset to *both* lateral arrangements of each pair of cards and trained to go to

one of the stripes and one of the grays, irrespective of lateral arrangement. All of the animals of the present experiment reached the criterion by the fourteenth day of training, at which time the animals of Saldanha and Bitterman were performing close to the chance level (Fig. 3). Furthermore, on the fifteenth day, when almost all of the latter animals were responding either randomly or in terms of position habits, the animals of the present experiment, shifted to Problem II, reacted differentially to stripe and gray situations.

### EXPERIMENT III

The development of qualitatively distinct levels of perceptual organization in the two- and four-situational problems is revealed most clearly under conditions in which the animals of the two groups have equal experience with the stimulus-cards. The third experiment of this series was designed to provide such a comparison.

*Subjects:* Twenty naive, male rats of the Wistar strain, ranging in age from 100 to 150 days were studied.

*Apparatus:* The apparatus was the same as that employed in Experiment II.

*Procedure:* The procedure of Experiment II was duplicated with one important exception. After each animal had achieved one errorless day on Problem I it was given 19 days of over-training on that problem before being shifted to Problem II.

### Results and Discussion

The results obtained in the third experiment resemble those obtained in the second. As the learning curves of Fig. 3 illustrate, the two groups of animals reached the criterion on Problem I by the fourteenth day (averaging 40.6 and 42.9 errors, respectively) and continued to perform at a high level of accuracy during the over-training phase (aver-

aging less than half an error per animal per day). As in Experiment II, however, Problem II distinguished the groups, the difference in this case being greater than that previously obtained. When the deviation of each animal's error score from the chance value (six errors) on the first day of Problem II is used as an index of situational generalization, the values obtained in the third experiment prove to be significantly higher than those obtained in the second (Festinger's method,  $p < .01$ ). In the context of a two-situational problem, therefore, *increased frequency of reward and punishment may impair rather than facilitate differentiation between the members of each pair of stimuli.*

The performance of the animals of Saldanha and Bitterman (14), which were trained in a four-situational problem involving the same cards, is plotted in Fig. 3. By the thirty-fourth day of training these animals were performing very close to the 100 per cent level—that is, they were differentiating consistently between the two members of each pair of cards. After thirty-three days of reinforcement and punishment on the same cards in the two-situational problem, the animals of the present experiment responded with almost equal readiness to each member of each pair, although they discriminated consistently between gray and stripe configurations. This evidence points unmistakably to the development of qualitatively distinct levels of perceptual organization under the conditions of training being compared. The phenomenon of situational generalization which is revealed in experiments with two-situational problems implies the existence of a process of perceptual organization which is non-additive in nature in the sense that it cannot be reduced to the acquisition of functional properties by afferent components (as in the theory of Spence)

and which is non-differentiated in the sense of the relational theory of Lashley or the general approach-avoidance formulation of Nissen (13). The data of this experiment and the data of Weise and Bitterman suggest that aggregations of afferent components may function initially as loosely organized wholes out of which the perception of objects and relations is subsequently differentiated. The nature of the transition from this primitive level of organization to more complex levels remains for subsequent investigation.

#### SUMMARY

An experimental situation has been developed for which the two major contemporary theories of discrimination learning—the conditioning theory of Spence and the relational theory of Lashley—lead to essentially the same incorrect deduction. The results clearly reveal the operation of a primitive level of perceptual organization which is both non-additive and non-relational in character—a diffuse, undifferentiated configurational process which is functionally prior to the perception of objects and relations.

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## VISUAL PERCEPTION AS INVARIANCE

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The railroad tracks stretch straight and far away from me over the desert and on to the horizon. I stand squarely between them, looking along them to the horizon, and I observe both that they converge as distance gets greater and also that they are at every distance equidistant. This is the perceptual paradox of converging parallels. Every one has the experience, and Blumenfeld (1, pp. 323-346) found it under the controlled conditions of his alley experiment.<sup>1</sup>

The convergence, when you have regard to it, is irresistible, and it is much less than the convergence of the retinal image that underlies the perception. That image might converge as much as the legs of an isosceles triangle that is almost equilateral. The base of the triangle, the tracks at your right and left, would be at the top of your inverted retinal image, and the lines of the image for the tracks would come together quickly, meeting at the fovea, on which would be the image of the vanishing point at the horizon. It is thus plain that the perceptual pattern is not the pattern of the retinal image nor any form topographically equivalent to it.

<sup>1</sup> It is to my colleague, Dr. S. S. Stevens (12), that I owe the thought that the concept of invariance can be given as much importance in psychology and biology as it has in mathematics and physics. He has criticized and improved this paper which now goes to the editor in its fourth draft. The notion that a stimulus is not something given in research but something to be discovered by research I did not get from Stevens but rather, thirty years ago, from John Dewey's famous reflex-arc discussion in 1896 (5). The motive for my paper was, of course, furnished by Gibson (3, 4).

It seems improbable, furthermore, that anyone ever observes the convergence and the equidistance of the tracks simultaneously. You can see the pattern one way or the other at will, according to which question you ask yourself about the perception. There must, therefore, be two *Aufgaben*, two attitudes, one for each of these observations. Certainly it is not safe, without further inquiry, to say that one observation is more primary than the other, or more immediate (quicker), or less inferential. Presently we must relate these two attitudes to dangerous concepts like *seeing* and *knowing*, but for the moment they remain merely two landmarks in a paradigm: (1) the perceived convergence and (2) the perceived equidistance.

The phenomena of perceived size with distance variant also furnish us with another paradigm. For free binocular vision, with enough of the normal clues to the perception of distance available to the observer, the rule holds that perceived size stays constant when distance changes, that is to say, the perceived size of an object is invariant under the transformation of the object's distance from the observer, while retinal size is, of course, variant under this transformation. If the available clues are, however, reduced enough, then the perceived size comes to depend more and more on retinal size and less and less on object size when distance is varied. With complete reduction, with complete elimination of the clues to distance, retinal size (visual angle) becomes the determiner, and object size can vary with distance without af-



fecting perceived size as long as retinal size stays invariant (6, 11).

These relationships hold up to about one hundred yards. What happens, we may ask, at greater distances, at 500 yards, or to the perceived six-foot man half a mile away across the valley? Common sense says that this man looks like an ant. Is it a six-foot ant that he looks like or a little one? Gibson's experiments assert that size-constancy does not fail at great distances (4, pp. 174-186). He showed that a six-foot pole half a mile away, with the intervening terrain clearly visible, is equated in perception to the six-foot pole close at hand, and I say, as Gibson did not, that the pole looks just as big *although* it looks smaller. You can judge it either way, for the paradox is comparable to the dilemma of the railroad tracks.

#### IMMEDIACY AND INFERENCE

The first question that arises about these paradoxes is whether there may not be two systems, two kinds of experience which occur with different points of view, that is to say, with different observational attitudes. The one system would include the converging tracks and the tiny man in the distance, the other would show size constancy. Titchener was always saying that the sciences observe the same experience but from different points of view (13, pp. 133-143, 259-266). Why may not an attitudinal difference in observation serve us here? Let us see. We shall need names for any such two kinds of experience, and a difficulty arises because every familiar term that is applied seems to prejudice the final outcome. We can, however, reduce this bias to a minimum by calling the system that includes the converging tracks and the little man the *System R* and the one that shows the size of perceived objects invariant with distance the Sys-

*tem O*. If I confess now that *R* seems to me to have something to do with *Reduction* and *O* something to do with *Objects*, you will see that I am begging the question, but not very much. You are still free to give other meanings to my symbols.

The first difference that suggests itself is the possible distinction between *immediate* and *inferential*, but this differentiation at once runs afoul of psychology's classical debate about the nature of experience. Wundt and Titchener would have said that sensations, contents, existential processes are immediately given, that objects, knowledge and meanings are secondary and derived from these givens. You get the givens immediately by description (*Beschreibung, cognitio rei*) and the derived entities mediately by inference (*Kundgabe, cognitio circa rem*). Titchener might have added that for the first you need *cues*, but for the second *clues*. Let us call this view the Leipzig view: Objects are made of contents.

The Gestalt psychologists, however, take exactly the opposite view. For them objects are found in immediate experience, whereas the sensations, contents and existential processes are psychologists' constructs, derived by inference and abstraction from direct experience. The immediately given are called phenomena, not contents, and phenomena are objective in their very essence. Köhler, distinguishing between value and fact, complained that the introspectionists limit themselves to the use of "concepts which have acquired a certain polish in the history of scientific thought, and," he added, "they think little of topics to which these concepts cannot be directly applied" (9, p. vii). Experience, the Gestaltists hold, is organized into objects from the first instant of its availability. Let us call this view the Berlin view: Contents are extracted from objects.

Now let us contrast the Leipzig with the Berlin view in respect of perceived size with distance variant. Leipzig says that you can see that the distant stick is smaller than the near but that you know it is just as big. Berlin says you can see that it is just as big but that you know it ought to look smaller. There is, however, an eclectic view, in which it appears that the immediate datum sometimes corresponds with the object, sometimes with the reduced sensory core of the perception, and is sometimes intermediate. Given enough clues to distance, size constancy ordinarily holds for an object placed at different distances within a couple of hundred feet of the observer; yet it may well be, as Gibson suggests, that a skilled artist can "see" or at least infer the size that he should give the object in a drawing on paper, a size that corresponds, of course, to the size of his retinal image and not to the actual constant size of the object. Conversely, an observer may see a distant man as quite small but infer that the fellow must nevertheless be a six-footer. Gibson does not say whether his stick, a half mile away, looked to his observers as small as it looks to him who observes the photograph of it in Gibson's book (4, pp. 184f.), whether his observers then inferred that, small but distant, it must match a six-foot pole nearby, or whether, on the other hand, they made their judgments immediately and with assurance. Certainly they may have done so. Even on the Leipzig view the perception of an object—the perception that Titchener called the "stimulus-error"—is often easy and quick (2, pp. 460-470).

A still better example for showing the need to compromise between the two extreme views lies in the perception of the size of the full moon's disk. The moon, 240,000 miles away, subtends a visual angle of about 0.5 de-

gree, but the disk of light 12 feet away, the disk whose perception matches the moon's perception in size, is never, even with the moon looking small in elevation, less than 1.5 degrees (a diameter of about 4 inches). In short, two retinal images give rise to two perceptions that are equal in size when one image is three times as large as the other in diameter, or nine times as large in area. This is a deviation in the direction of object size constancy, but it does not go very far in this direction. If size constancy held, this disk 12 feet away and only 4 inches across ought to look as if it had a diameter of 2160 miles (8). It does not. The moon, a very distant object, does not look nearly so big as it would if it were close by. In other words the moon, an object, does not get itself perceived in the System *O*, the Berlin system. Is there some kind of a system *R* into which it fits? If there is, certainly that system is also not going to be one in which sizes are proportional to retinal sizes.

#### THE VISUAL FIELD AND THE VISUAL WORLD

Perhaps Gibson's distinction (3, 4) between the visual field and the visual world will give us the systems we are looking for. What is this field? and this world?

The visual world is the easier to understand. It is what Berlin has been calling the world of phenomena, and thus the world of perceived objects, the Gestalt world of perception, an unbounded, stable, rigid, Euclidean world, always tridimensional, with parallels always equidistant—in fact the natural world of objects duplicated in perception. Since objects do not change in size when moved, the perceptions of moved objects do not in this world change in size. Object constancy is the rule in the phenomenal perceptual world because it is the rule in the "ex-

ternal" natural world. In short, evolution appears to have achieved an organism in which perception duplicates or at least takes adequate account of the real external world, within small tolerances, and with only a little illusion and error. As usual, however, it is the exceptions, the alternatives, the illusions and the errors that claim our attention.

The visual field is offered us as one alternative. It is not for Gibson the visual world. It tends to be bidimensional, pictorial, and in a sense "anatomical" like the retinal image. Yet it is certainly not the retinal field, for the visual field is never doubled in binocular vision, as is the retinal field, nor is it as diplopic. The field, unlike the world, is limited in extent, changing, fluid and non-Euclidean, as you can see if you study its flow, expansion, distortion and contraction as its observer flies rapidly through it in an airplane. If the visual world is made of perceptions, perhaps the visual field is made of sensations; yet Gibson, in suggesting the appropriateness of these two classical terms, does not mean that the visual field is prior to the visual world, the basic inventory out of which the object world is made. I believe Gibson would place the converging railroad tracks and the little distant man in the visual field, because he suggests that the visual field may actually be seen by the trained artist or introspective psychologist, who can abstract from objectification and see experience as . . . as it really is? Well, at least as it really is in the visual field.

The visual field is, of course, not the brain field either. It might be isomorphic with the brain field, but that we cannot say. Here we are looking for full topographical correspondence, not mere topological identity, for a correspondence of sizes, directions and distances. The visual field must come

nearer matching a monocular retinal field than the cortical field which is divided between two hemispheres.

These distinctions leave us Gibson's visual field, freely suspended *in vacuo* with full freedom to be itself. It is not the perceived visual world of objects, nor the visual projection field in the cerebral cortex, nor the retinal field, nor the pattern of optical projection on the retina, nor the pattern of the world of external objects itself. It has its own properties, rules and limitations. Certainly it is no longer possible for any of us to go along with Wundt and Titchener and to say that the visual field is immediately given. The world of objects (or of stimuli, as Titchener would have called them) can appear as promptly and as fully organized as can that specially edited experience that the trained introspectionist and the artist learn to see, perhaps at times with as much celerity as they can see the stone that Dr. Johnson kicked. Nevertheless there is a use for Gibson's visual field as well as for his visual world, although both concepts are in need of further specification. At present these two systems float freely in a parallelistic pluralism, and they can be given—it seems to me—more precise meaning and better specification by operational reduction. Let us see what operationalism can do for them.

#### PERCEPTION AS INVARIANCE

More than fifty years ago John Dewey remarked that one problem of stimulus-response reflexology is the discovery of the stimulus (5, pp. 367-370). He was right, for the effective stimulus is not an object but a property of the stimulus-object, some crucial property that cannot be altered without changing the response, some property that remains invariant, for a given response, in the face of transformations of other characteristics. Since then scientists

have been coming to realize, as Stevens points out (12, pp. 19-21), that the discovery of invariances can be regarded as the chief problem of a quantitative science that has passed beyond the stage of phenomenology. And it is in terms of invariance that perception can be specified operationally.

Again let us consider the case of perceived size. What is perceptual size constancy? It is the rule that perceived object size is invariant under the transformation of tape-measured distance and thus also under the transformation of perceived distance, since tape-measured distance and perceived distance are known to vary together. There is another rule which goes along with this one, a fact that we take for granted and do not often state in psychological context. It is the rule of physical size constancy, the rule that tape-measured object size is invariant under the transformation of tape-measured distance or other change of location. Objects do not shrink or expand as you move them around, and neither do your perceptions of them when you have those conditions of no-reduction under which size constancy occurs. We have, under these circumstances, the correlation of two similar invariances, the invariance for physical size and for perceptual size, and we are free to imagine, if we wish, that evolution aimed at this achievement, making perception adequate to reality in order to increase the organism's chance of survival.

A less dualistic way of stating this relation is as follows. You can determine the invariance of the size of objects under the transformation of location either (a) by the direct comparison of the object in one place with the object in another or (b) by indirect comparison of the object in different places through the mediation of a tape-measure. In the latter case you

compare the object directly with the marking on the tape, and you can keep distance constant by always reading the tape at a fixed distance. A great deal of other evidence also contributes to the accepted theory that objects do not change size appreciably when they move around on the face of the earth with ordinary velocities. The rule of size constancy thus becomes this: Under the transformation of location, size observed by direct comparison is invariant when size observed by tape-measuring is invariant. In short, we have two invariances correlated. There can be no mistake about there being two, for one breaks down more easily than the other. Reduce the clues to distance, and the correlation no longer holds, for then receding objects are seen to shrink, although not to recede.

Size constancy, defined operationally by this correlation of two observed invariances, can be translated into the common-sense statement: A man (or a chimpanzee) can perceive correctly the physical size of an object. The perceiver can perceive in direct comparison whatever remains invariant under the transformation of distance. We may next properly ask: Can a man (or a chimpanzee) also perceive the size of his retinal images, that is to say, can he be an artist or an introspectionist? Perhaps the man can though the chimpanzee can not. We need to know exactly what observation would demonstrate that an organism is perceiving the size of its own retinal images.

For a man to perceive the size of his own retinal images his perception of size must remain invariant under all transformations that leave the size of the retinal images invariant, including the crucial transformation involving object distance. If  $s$  is the linear size of the object and  $d$  is its distance from the eye, then retinal size (visual angle) is

invariant when  $s/d$  is invariant, so the question becomes: Can the artist or introspectionist acquire and use an observational attitude under which perceived size stays fixed whenever  $s/d$  remains invariant, even under the transformation of distance? Human artists can come near to maintaining this invariance, but there are conditions under which the relation breaks down.

It breaks down, for instance, in perceiving the moon. As we have already noted it is impossible to perceive the moon as big as it really is (2160 miles across) or as small as its retinal image is (0.5 degree across). You see something in between, nearer retinal size than object size (8). Certainly when celestial distances are involved neither object size nor retinal size determines perceived size. What is needed is the discovery of the size-invariant for celestial distances, the discovery of the stimulus. We might know how properly to specify the stimulus if we knew the actual sizes of many moons that, at different distances from the earth, all look the same size. How big must moons that look alike be if they are a thousand miles away and a million miles away and at many distances in between, including the 240,000 miles that our regular moon is distant? The graph of those data would disclose the law of invariance, a statement of what is perceived under the attitude for judging size at great distances. If we could find a function,  $\phi$ , that would be invariant when perceived size is invariant—an expression in terms of actual distance, perceived distance, actual elevation of the moon, elevation of the observer's regard, observer's attitude, and any other parameters that turned out to be essential—then we could say even better what it is that is being perceived (invariant). In short, if perceived size is invariant when this function,  $\phi$ , is invariant, then, in judging size, you are

perceiving not object size, not retinal size, but  $\phi$ . To discover the object of perception, you have to discover what function of the parameters of the stimulus is invariant when the perception is invariant. That is a good operational definition of perception in terms of stimulus invariance.

#### THE VISUAL FIELD AND PERCEPTUAL REDUCTION

Gibson is writing phenomenology and he tells us that we have a visual world that corresponds in general with considerable accuracy to the rigid, Euclidean, natural, tape-measured world, and with but small exceptions for illusion and error. That is good phenomenology and natural philosophy, but it is not the body of exact quantitative knowledge that we call science nowadays. Just as the scientific physics of the natural world, with its molecules, atoms and electrons, is not something that you can look at and see, so the scientific psychology of the visual world differs from phenomenology in being a collection of observed functional relations that can be approximately summarized by the hypothesization of a Euclidean model. You cannot see the visual world at any moment when you are playing scientist; you construct it out of elaborate observations that have been being collected for many years in the past.

Gibson's visual field, a concept that creates difficulty even in phenomenology, seems to me to become clear in terms of our examples—the converging tracks, the little man in the distance, the moon that is both too big and too small. I think Gibson would accept these items as belonging in the visual field, but no matter. Let us put them in our own System  $R$ , and now let us come back to what we were planning to do all along; let us say that the System  $R$  is a system of *reduced vision*.



Our examples are all instances of partially reduced perceptions of visual size with distance variant. The System *R* (and perhaps Gibson's visual field?) is the reduced visual world, the totality of those simpler sights where reduction of the total complexity of clues makes the observer dependent upon but a few parameters of the stimulus or perhaps upon only a single one, like retinal size. For this system *R* there is no obvious model, like the Euclidean model for the visual world. The System *R*, the field of reduced perceptions, is simply a congeries of observed invariances. These reductions are, moreover, not always complete. There are limits to what attitudinal abstraction in observation and to what experimental control can accomplish in the elimination of clues. If reduction were indeed complete, then the System *R* might come to resemble or even to duplicate the retinal field. In fact, it becomes clear that these cases of partial but incomplete reduction are the occasion for the present paper.

Now let us consider another case of incomplete reduction, the case of binocular vision. Can a man tell with which eye something is seen? Presumably a pigeon can (10), but for a man the answer is yes and no. His brain knows one eye from the other as it translates retinal disparity into perceived depth. His verbal mechanisms know the difference only after he has tried first shutting one eye and then the other. He can see depth based on disparity when he cannot see diplopia. Complete reduction of binocular vision would be a reduction not to a retinal image but to two retinal images. So we have here, if we are thinking of the artist's view, another instance of the partial but incomplete integration of the physiological pattern into the perceived pattern, a crucial example where perception lies intermediate between com-

plete "reduction" to the retinal image and complete "regression" to (integration of) the real object.

In general it seems to me better not to try to create a model for the System *R* (or the visual field), but to leave these facts as they were born, in an inventory of invariances under various reductions. The invariances tell us what the organism can do under attitudinal training to perceive its own physiological bases, the data out of which it can, after much evolution, create an extremely useful apprehension of the world that it accepts as its reality.

Let me not seem to belittle phenomenology nor our debt to Gibson. Phenomenological description is a valuable *vorwissenschaftliches* undertaking. It shows what the psychological problems are. This paper of mine is concerned with indicating the nature of the next step beyond phenomenology and with demonstrating how the scientific problems of perception can be pushed forward by a study of the parametric invariances of the stimulus.

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## THE VISUAL FIELD AND THE VISUAL WORLD: A REPLY TO PROFESSOR BORING

BY JAMES J. GIBSON

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Let us begin with the railroad tracks extending to the horizon. They are "seen" in one sense of that term to converge; they are "seen" in another sense of that term *not* to converge. The former appearance is what I call the visual field; the latter is what I call the visual world, and the hypothesis is that there exist, as limits, two correspondingly different kinds of seeing. By adopting the appropriate attitude, one can have either kind of visual experience. So far, Professor Boring and I agree.

His suggestion is that we think of the first mode of perception as "reduced" and the second mode as "objective," taking into account the fact that with complete elimination of the clues to distance phenomenal size appears to be reduced to angular or perspective size. The experiments reported by Holway and Boring (2) do indeed indicate as a fact that we tend to see in perspective when there are no clues to the distance of the critical object. But I have come to wonder whether this is a fact, i.e., a necessary and universal fact. Is it not possible that, when there are no stimuli for the perception of distance, the impression of size simply becomes *indeterminate*, along with the impression of distance? One might then suppose that in the experiments so far performed *O*s have found it easy to adopt the perspective attitude—very much easier than it is when stimuli for distance are present. If the conditions of an experiment are such that *O* can see the critical object *either* large and far *or* small and near (like the other object with which it is to be com-

pared) he will probably tend to see it in the latter way. In this interpretation, the reduction of depth-cues does not *make* us see depthless sensations; it only *enables* us to see in perspective if we are set that way. "Reduced" vision is not any more primitive than ordinary vision with full stimulation; it is simply less determinative of space-perception.

This point is important because Professor Boring wants to define the visual field as a case of reduced visual stimulation, and suggests this as the operational definition of the experience in question. He is clear and explicit, and this makes me clarify my own assumptions. I am pretty sure that I disagree. The visual field, I think, is simply the pictorial mode of visual perception, and it depends in the last analysis not on conditions of stimulation but on conditions of attitude. The visual field is a product of the chronic habit of civilized men of seeing the world as a picture. In the case of the railroad tracks, it is what the scene looks like when *O* attends not to depth but to the clues for depth. In the case of the size-constancy experiment with impoverished stimulation the visual field is what *O* can most easily report, since the stimulation is wholly indeterminate as between large-far and small-near.

The visual field, then, cannot be given a complete operational definition in terms of stimulation alone but only in terms of response to stimulation. The experience of the field and the experience of the world, in the case of the railroad tracks at least, are alternative modes of response to the same stimula-

tion. In general, I suspect that overt locomotor behavior is bound up with the latter mode of response (the world) whereas verbal behavior (introspection) may be bound up *either* with the field mode or the world mode. Until recently, however, most verbal descriptions in philosophy and psychology have been of the visual field only. Hence arises the fact that we have an established psychophysics of color stimulation but, as yet, no psychophysics of spatial stimulation.

Professor Boring contrasts the theories of perception issuing from Leipzig and Berlin with neatness and elegance, and he rightly assigns me to the camp of the Berliners. But on this issue of whether sensations are primary and objects secondary, or whether objects are primary and sensations secondary he prefers to be eclectic, and feels the need of compromising between the two extreme views. To search for such a compromise may be the part of wisdom, but I cannot myself see where it is to be found. One view implies a "clue-theory" of the perception of objects and the other suggests (to me) a "stimulus-theory" of the perception of objects. I should prefer not to soften the issue, but to adopt the latter theory for the reason that it may enable us to develop a genuine psychophysics of object-perception.

Unless I misunderstand him, Professor Boring is defending the notion that perception is a process intermediate between sensation and knowledge. But this is precisely the doctrine which I should like to question. We should seriously consider the possibility that the classical concept of visual sensations is, and always has been, a snare and a delusion. There are variables and entities of visual experience, true enough, and it is probable that the child starts life with few and the adult ends up with many, but to slice this hierarchy in the traditional fashion leads to all kinds of theoretical trouble.

The way to begin an experimental science of perception, I suggest, is to investigate *all* the discriminable properties and qualities of visual experience, not those of color only, and to find out first whether they correspond to variables of complex stimulation. The method is the psychophysical experiment. This has to be supplemented with an investigation of the identifiable objects of experience from the vaguest and least familiar to the most specific and differentiated. The method here consists of employing the standard learning experiments to study the physical objects to which an individual's behavior is specific. These physical objects are reacted to, of course, only because they are specific sources of proximal stimulation at receptors.

This brings us to the concept of invariants (or invariances) in perception and in stimulation. With most of what Professor Boring has to say about it, so far as I understand him, I agree enthusiastically. But I should like to go a step farther than he does and apply the concept, in a speculative way, not only to the measurements of a physical object in different places and to the size-judgments of the perceived object at different distances but to the complex of proximal stimulation itself. The proximal stimulus for vision is a bifurcated array of light-energy analysable into margins and textures and, at a higher level, into gradients of texture, gradients of disparity, and gradients of deformation (1). Let us assume that the gradients yield a corresponding impression of distance. Let us also assume that a pair of congruent bounded areas yields a figure-ground impression. Something like the following hypothesis is then possible: When expansion of the bounded areas relative to the whole array goes with a coarsening of their texture relative to the whole array, with an increase in their crossed disparity, and with an increase in their crossed

motility (and when contraction goes with the opposite variations) an *invariant of retinal stimulation exists*. As a given object moves toward or away from the eyes of *O*, accordingly, there is a *resultant variable of stimulation not affected by distance to which the phenomenal size of the object may correspond*. The constancy of size of the perceived object is then a result of a constant value in retinal stimulation. We cannot compute this value at present, or even give it a name, but its existence is reasonable.

Evidently a psychophysics of the constant properties of phenomenal objects—size, shape, and probably also color—is at least theoretically possible. It is tempting, of course, to leave the proximal stimulation out of account in perceptual research, and to skip from the properties of the percept to the properties of the object, the “distal stimulus.” In effect this is what many constancy experiments do; it is exemplified in many experiments on the accuracy of estimation, and it is done in all learning experiments where “stimuli” mean “objects.” But a psychophysics based on the properties of objects has no solid foundation; it simply begs the question. What we need is a psychophysics based on the properties of stimulation to which physical objects are specific—the invariants of stimulation.

To conclude, Professor Boring's shrewd discussion has prodded me into an effort at theoretical consistency. I wish to propose the following explicit assumptions about size-perception:

1. In the case of normal everyday vision the stimulus for phenomenal size is always *dual*, i.e., it is jointly a retinal area (or rather a pair of them) and a set of distance-stimuli. Size-constancy results from an invariant of these two concomitants.

2. As a corollary, when distance is not determined by stimulation, their size also will be indeterminate.

3. When distance is not determined by stimulation, it may be determined by an attitude, that is, by some *presumed* distance on an imaginal plane vaguely in front of the eyes.

4. The phenomenal impressions of size and distance are inseparable; they are more or less rigidly linked and consequently when a presumed distance arises in experience a presumed size accompanies it. This is the impression of perspective size. (It should not be confused with *retinal size*, which is not an impression but a physical measurement.)

5. The visual field is a picture-like phenomenal experience at a presumptive phenomenal distance from the eyes, consisting of perspective size-impressions. These size-impressions are determined by the areal stimuli conjoined with the presumption. The visual field is *not* a copy of the retinal stimulation, and is not even similar to the retinal stimulation as all of us have been taking for granted.

6. The effect of stimulus-reduction on object-perception is to substitute for the normal perceptual process of size-determination an attitudinal process. The resulting pictorial impression is not the *basis* of ordinary perception. It is merely a convenient simplification for purposes of research on the sensitivity of the single retina. So far from being the basis, it is a kind of *alternative* to ordinary perception.

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# MATHEMATICAL FORMULATIONS OF LEARNING PHENOMENA \*

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## I

The present paper is concerned with some problems that arise in connection with the attempts of psychologists to formulate precise quantitative theories about learning phenomena. Before turning to the mathematical aspects of our topic, however, I should like to do two things: (1) to discuss, very briefly, the experimental phenomena with which we are to be concerned and (2) to consider the purposes that theories serve at the present stage of development of the field of learning. The experimental studies on learning that have provided data sufficiently precise to invite the use of mathematical functions in their description and interpretation have employed relatively simple behavior situations such as classical and instrumental conditioning, simple trial-and-error learning, discrimination learning and serial or maze learning.

The basic data provided by these different kinds of learning experiments consist in a set of empirical functions relating various response measures to a number of experimentally manipulatable environmental variables. The following represent some of these discovered relationships for one experi-

mental situation, classical conditioning:

- 1)  $R = f$  (Number of trials— $N$ )
  - 2)  $R = f$  (Intensity of the conditioned stimulus— $S_c$ )
  - 3)  $R = f$  (Intensity of the unconditioned stimulus— $S_u$ )
  - 4)  $R = f$  (Time interval between  $S_c$  and  $S_u - T_{s_c-s_u}$ )
  - 5)  $R = f$  (Time between successive trials —  $T_R$ )
  - 6)  $R = f$  (Amount of work involved in  $R - W$ )
- $R = f(N, S_c, S_u, T_{s_c-s_u}, T_R, W, \text{etc.})$

The so-called learning curve, representing the changes that occur in the performance measure as a function of the successive practice occasions, is listed as the first of these functions. While this is the function or law in which learning psychologists have shown the most interest, the other relationships are equally important for the complete description of the behavior of the subject.

As the result of our experimental studies, then, we arrive at a series of empirical laws relating each response measure in the various types of learning situations to  $N$  and to the several other determining variables. Assuming that we can obtain such sets of laws for each of the learning situations, why, one may ask, do we introduce theories? What do theories add or what do they provide that the sets of laws do not?

There are psychologists who take the position that all we need to do is to discover such sets of empirical

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laws and that theorizing, at least at the present stage of development of the field of learning, is not necessary. Actually, if one were satisfied to confine one's study of learning phenomena to *one* particular response measure in a *single* experimental situation, e.g., the frequency measure in classical conditioning, there possibly would be no need for theory. Thus, if it were, in fact, found that a single equation fitted the various curves of frequency obtained in the conditioning experiment under different values of the other experimental variables ( $S_e$ ,  $S_u$ ,  $T_{g_u}$ , etc.), then one would have a single law that consistently and adequately described all of the curves.<sup>1</sup>

But now let us suppose that when we employed other measures of the conditioned response, e.g., amplitude or resistance to extinction, we found that the curves of learning for these measures took quite different forms from that of the frequency measure. Or suppose on turning to other experimental situations such as the discrimination box, the maze, etc., we found that still different types of learning curves were obtained. We would thus have a series of more or less specific laws for each particular learning situation and, in some instances, even different laws for each different response measure in the same experimental situation. Anyone familiar with the nature of learning data at the present time will readily recognize that this picture is by no means a construction of my imagination, but represents a fairly accurate portrayal of the existing state of our knowledge in this field.

<sup>1</sup> The fact that such a psychologist as Skinner (9) finds little or no need for theory in learning is probably not unrelated to the fact that he has confined his interest in learning data largely to one measure in a single learning situation, i.e., rate of responding in operant conditioning.

Confronted with such a state of affairs, the theory-oriented psychologist has attempted to integrate these isolated, particular sets of laws into a more comprehensive system of knowledge by means of his theoretical formulations. The more empirical-minded psychologist, on the other hand, has typically not been interested in such integration, believing such attempts to be premature and wasteful at the present stage of development of knowledge in the field. There is, of course, no recipe or set of rules that will tell us precisely when any realm of empirical facts is ready for such attempts at theoretical integration. Undoubtedly, differences among psychologists in regard to this predilection for engaging in theory construction reflect differences in personal attitudes, special skills, etc., that lie quite outside the scope of the present discussion. Most learning psychologists will be found to fall somewhere in between the radical empiricism of Skinner (9) and the sometimes purely mathematical model building of Rашevsky (8).

One of the most highly developed quantitative theories of learning phenomena, at present is that of Clark Hull (6). Basing his theory on data from classical and instrumental conditioning experiments, Hull has been engaged for a number of years in an attempt to show how the particular laws found in the different learning situations may be derived from this theoretical structure. Other quantitative theories similar in principle to Hull's, in that they are based on data from learning experiments themselves rather than on experimental findings in other fields such as neurophysiology, etc., are those of Thorstøne (12), Gulliksen and Wolffe (4), Graham and Gagné (2), Pitts (8), Estes (1) and Spence (10).

A second type of quantitative theorizing that has developed in the field of learning has had a quite different origin. Instead of being instigated by the diversity of curves of learning obtained in different types of experiments, this kind of theorizing has attempted to develop a mathematical theory based on neurological foundations. I have reference, of course, to the work of Rashevsky and his students (8). These two theoretical approaches do not, as is sometimes thought, represent competing formulations but are complementary to each other. The development by the behavior theorists of a more comprehensive theory consisting of a fewer number of general principles instead of a multitude of diverse laws that have no obvious relation to one another simplifies the problem for the neurophysiological theorist. Instead of having to derive a number of diverse experimental facts based on special conditions, he can direct his theory to the derivation of these more general learning principles.<sup>2</sup>

<sup>2</sup> Considerable confusion has arisen from a failure to realize that these two types of quantitative theories of learning phenomena are, or can be, entirely independent of another class of learning theories, namely, those concerned with the nature of the reinforcing process. Whereas the former theories attempt to provide guesses as to the laws governing the course of development of the hypothetical learning changes that occur with successive practice occasions, the latter are concerned with the conceptions as to how the unconditioned or reinforcing stimulus provides for the hypothetical change. The mathematical learning theorist can employ any one of these latter conceptions he wishes or he can completely ignore them. Thus he can be a reinforcement theorist of whatever variety (need-reduction, drive stimulus reduction, satisfier, etc.) or a contiguity theorist of whatever type he desires.

The almost identical nature of the mathematical portion of Estes' treatment (1) of classical conditioning within the framework

With this discussion of the nature of learning data and the general aim or purpose lying behind the attempts at quantitative theorizing about them as background, I now wish to turn more specifically to the role of mathematical functions in the description and interpretation of learning phenomena. We shall not stop to discuss at any length the fitting of learning curves by empirical equations. In the decade following World War I there was a flurry of such activity on the part of psychologists and a number of different mathematical functions were employed, among them the hyperbola, arc-cotangent, Gompertz, logarithmic, logistic and exponential functions. For the most part these equations were selected merely because of a resemblance between the learning curve and the mathematical function. However, the logistic and exponential functions were favored not only on this basis but also because their proponents believed that they provided a kind of explanation of the learning process. Following the reasoning of some of the biologists in their treatment of similar curves of body growth, these psychologists postulated either (1) that the learning process was one in which the rate of development of the process was proportional to the amount still to be developed, or (2) that the rate of learning was proportional to the product of the amount already developed and the amount of the process still to be developed. Integration of the first of these assumptions (both of which can be expressed as differential equations), leads to the ex-

of Guthrie's contiguity position with the mathematical portions of Hull's reinforcement treatment points up convincingly the independence of these two areas of theorizing in learning.

ponential function;<sup>3</sup> integration of the second leads to the logistic function.<sup>4</sup>

Such "deductions" of the empirical curve of learning do not, as some psychologists seem to have thought, represent any real advance in our knowledge of the learning process. Actually such "theoretical" treatments, whether they begin with the differential equation or start directly with the integral function, represent *ad hoc* assumptions that both begin and end with the original empirical curves. A genuine theoretical attempt to account for these learning curves would begin with assumptions concerning underlying hypothetical factors that lead to, rather than follow from, the original learning data.

That mathematical theories, the basic assumptions of which have their origin in laws concerning neurophysiological processes, offer the possibility of providing satisfactory noncircular explanations of learning data is readily accepted by almost all psychologists, regardless of whether or not they have any understanding of the mathematics involved. There is, however, less readiness to accept mathematical theories of learning that do not make reference to any underlying physiological mechanisms, but instead introduce hypothetical constructs or intervening variables (e.g., habit strength) as mathematical functions of the variables of learning experiments themselves.

<sup>3</sup>  $Y = a(1 - e^{-iX})$ , where  $Y$  = some measure of attainment or performance,  $X$  = measure of practice,  $a$  = limit of attainment,  $i$  = parameter determining rate of approach to attainment asymptote.

<sup>4</sup>  $Y = \frac{b}{ce^{-abX} + 1}$ , where  $Y$  = some measure of attainment or performance,  $X$  = measure of practice,  $a$  = parameter dependent upon individual learner and/or task to be learned,  $b$  = limit of attainment,  $c$  = constant of integration.

When one examines the objections given to this latter intervening variable type of mathematical theory, the one most frequently met is similar to that just given in connection with our discussion of the interpretations based on the properties of empirically fitted learning curves, namely, that they are purely *ad hoc* or entirely circular in character. They start and end with the same empirical data. Such an objection to the intervening variable type of learning theory, however, reveals a serious misunderstanding of its nature and purpose. I should like to attempt to correct this misunderstanding and to outline the nature of this type of theorizing as I understand it.

It is true that this kind of theory does begin with learning data, including curves of learning. But the theory does not stop with the treatment of the data on which it is based. To do so would, of course, leave it open to the criticism that it is purely an *ad hoc* affair that begins and ends with the same empirical data. Once formulated on the basis of one set of learning data, however, such theories are subsequently applied to other data either from the same situation or other learning situations. Rational equations representative of relationships to be expected in the new data are derived on the basis of the original constructs and principles. If the empirical findings do not agree with these derived equations, the theory is shown to be wrong and it must either be abandoned or modified in some manner. Any modification to meet the new data must, of course, meet the test of working satisfactorily for the original phenomena.

The particular type of learning that one selects as a basis for the beginning of such theorizing is, of course, purely arbitrary. On the assumption that

the simplest kind of learning situation is probably the best source in which to discover a set of basic constructs and principles that not only will work for these data but also serve as a basis for accounting for other learning experiments, Hull and I have started with the data from simple conditioning studies (conditioning, extinction, generalization, etc.). We have assumed that this type of situation provides the best source of evidence for making inferences as to the course of change that occurs during practice in the strength of a hypothetical stimulus-response connection or excitatory tendency. The more complex learning situations, it is assumed, are complicated by the presence or competition between a number of simultaneously occurring excitatory tendencies; hence data from them reflect only very indirectly the changes that occur in these *S-R* tendencies.

It should be noticed, however, that curves of classical conditioning do not provide an unequivocal picture as to their form. While some are negatively accelerated, others, particularly those using frequency of response as the measure of performance, show an initial phase of positive acceleration followed by a negative phase. Which of these functions are we to choose as representing the course of development of our hypothetical learning construct (habit, associative strength, etc.)? Unfortunately, a somewhat incorrect impression of the procedure that is followed at this stage of theory construction was gained by some psychologists as the result of Hull's treatment in his *Principles of Behavior*. On the basis of three experimental studies from his laboratory that had provided negatively accelerated curves of learning, Hull decided to assume that habit strength ( $sH_R$ ) develops according to this type of function. Actually, of course, even if every ex-

perimental study gave conditioning curves in which the *response measure* increased in some particular manner, it would still be entirely possible for the theorist to assume that some other function described the development of his hypothetical learning factor ( $sH_R$ ). As a matter of fact, in an earlier theoretical attempt Hull chose to assume a linear function in the face of essentially the same experimental evidence.

The point is that in postulating this hypothetical learning process the theorist is free to choose whatever assumption he wishes. Actually the theoretical model typically consists of a number of assumptions, and it is the implications of the complete model (not one particular portion of it) that must agree with the selected data from which the theory starts.

Having fashioned his theoretical model on the basis of one particular set of experimental data, the theorist, as described earlier, must now attempt to apply it to new data and new situations. Ideally this would involve the derivation of rational equations representative of relationships to be found in the new situation on the basis of the same hypothetical constructs and postulates employed in connection with the original data. While this is possible in some instances, as one attempts to apply the theoretical model to more and more complex situations, additional assumptions involving newly introduced experimental variables usually become necessary. One of the major problems faced in such theorizing is to find a way to introduce these new assumptions on some other than a purely *ad hoc* basis. When this cannot be done and the theorist makes the necessary new assumption such that it will account for some of the new findings, then the theory must again be tested by employing this new assumption to

predict other findings in the same or similar situations. The new assumptions must also be introduced without altering the old ones except as the new variables are assumed to produce interaction effects that would change them.

The nature of this type of theorizing may be shown by the following development of a theoretical model based on data from classical conditioning. Our treatment is patterned closely after that developed by Hull in his *Principles of Behavior*. Figure 1 presents the variables, experimental and hypothetical, that are involved. At the top are shown some of the experimental variables that have been shown to affect response strength in classical conditioning experiments. We are primarily interested in the relation assumed between  $H$ , the hypothetical learning change, and  $N$ , the number of conditioning trials.

We have followed Hull in postulating that the function relating  $H$  to  $N$

is the exponential,  $A(1 - e^{-bN})$ .  $A$  and  $b$  are parameters that determine, respectively, the limit to which  $H$  will grow and the rate at which it approaches this limit. Presumably these parameters vary for different individuals, i.e., fast and slow conditioners. We shall assume that the conditions determining inhibition,  $I$ , are negligible and hence can be ignored. In such a response as the eyelid there is probably very little work inhibition involved, especially if the intertrial interval is not too brief.

The variables  $S_c$ ,  $S_u$ ,  $T_{S_c-S_u}$  are assumed to determine a hypothetical construct,  $D$ , that we shall term drive level.  $D$  and  $H$  are assumed to multiply each other to determine, after subtraction of any  $I$ , the intervening variable,  $\bar{E}$ , effective excitatory potential. Finally, one further hypothetical factor, an oscillating inhibitory factor designated by the symbol  $O$ , is postulated. This oscil-

# EXPERIMENTAL VARIABLES

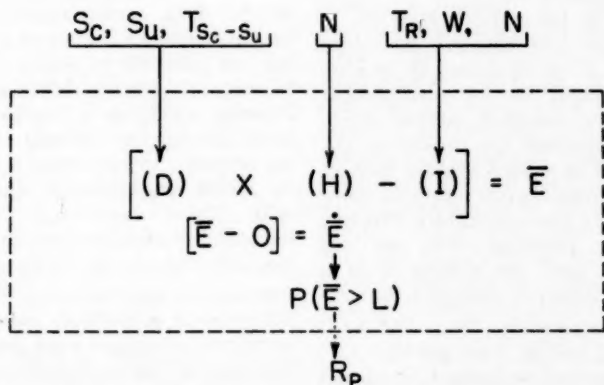


FIG. 1. Showing the relation between the experimentally manipulatable variables in classical conditioning, the hypothetical intervening variables in the rectangle, and the empirical response measure, per cent of conditioned responses  $R_p$ .



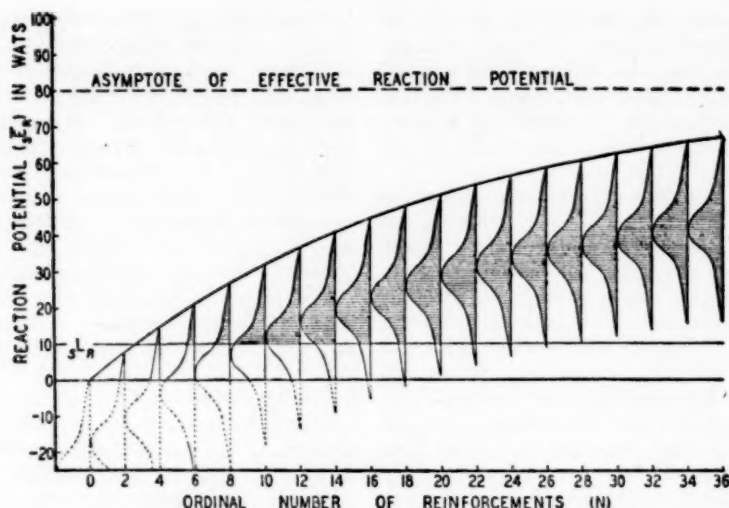


FIG. 2. Hull's diagram showing how with growth of effective excitatory potential, the proportion of superthreshold, momentary effective excitatory potentials [ $P(\bar{E} > L)$ ], represented as shaded portions of the upended normal distributions, increases.

latory potential is assumed to vary in amount from instant to instant according to a normal probability distribution, the range and sigma of which are constant. It is subtracted from  $\bar{E}$  to give  $\bar{E}$ , momentary effective excitatory potential.

Figure 2, taken from Hull (6, p. 327), shows how effective excitatory potential,  $\bar{E}$ , is conceived to develop as a function of the conditioning trials. The upended normal distributions represent the oscillatory potential. The shaded area in each of these distributions represents the probability that the momentary effective excitatory potential will, on the particular trial, be greater than a threshold value,  $L$ , necessary for a response occurrence, i.e.,  $P(\bar{E} > L)$ . Returning to Fig. 1 we see that this final theoretical variable,  $P$ , is identified or coordinated with the empirical response measure, frequency or percentage of response occurrences ( $R_p$ ).

If we plot these hypothetical  $P$  values as a function of  $N$  for a number of different values of the parameters ( $D$  and  $A$ ) that determine the level to which effective excitatory potential will grow, we obtain the family of curves shown in Fig. 3. In other words, these curves represent theoretical frequency curves of conditioning for subjects in which  $\bar{E}$ , either because of greater drive or better learning ability or a combination of both, develops at different rates. In an attempt to ascertain the extent to which experimental data agree with these theoretical frequency curves of conditioning we determined frequency curves for three groups of more or less like subjects. From 100 subjects run in an eyelid conditioning setup, three groups were selected on the basis of the total number of CR's made in 100 conditioning trials. The group curves for nine subjects (Group A) who gave between 71 to 80 CR's,



15 subjects (Group B) who gave a total of 50 to 58 CR's and 11 subjects (Group C) who gave from 32 to 40 CR's are shown in Fig. 4. As the differences between the subjects in each group are very slight, there is probably very little distortion resulting from the grouping of the data. Moreover, the form of the curve is not a product of the distribution of individual scores as is often the case in learning curves based on group data.

It will be seen that the data agree very well with the theoretical curves thus showing the applicability of the hypothetical model to them. It is, of course, possible to develop alternative sets of hypotheses that would fit the data equally well. The value of such theorizing, however, does not lie in the success with which it can fit the data on which it is based but rather in whether and to what extent it permits the derivation of rational equations that describe other empirical functions to be expected in this and other experimental situations.

As it stands, of course, the mathematical model described above is not sufficiently complete to provide pre-

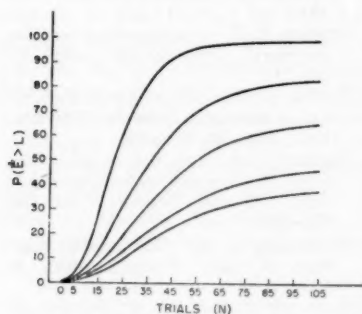


FIG. 3. Family of theoretically derived curves of the proportion of superthreshold, momentary effective excitatory potentials  $[P(\bar{E} > L)]$  as a function of number of training trials for different growth curves of excitatory potential ( $\bar{E}$ ).

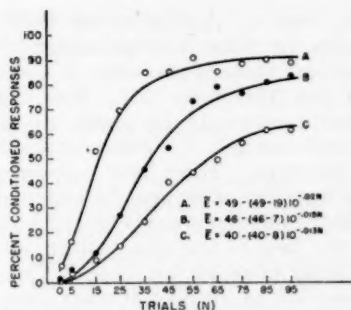


FIG. 4. Curves of conditioning for three groups of "like" subjects as described in text. The response measure is the per cent of conditioned responses occurring in successive blocks of 10 trials. The points on the abscissae represent the mid-points of the successive 10 trial blocks. The equations are exponential functions describing the growth of  $\bar{E}$  from which the solid theoretical curves passing through the empirical points (circles) were derived by means of a table of normal probability values.

dictions about other learning situations. It is presented here merely as an example of this type of model construction. Hull has gone considerably beyond the above described theory in that he has included hypothetical constructs and principles relating a number of other experimental variables, e.g., his assumptions about work inhibition, motivation, generalization, stimulus interaction, etc. On the other hand, it should also be emphasized that in his *Principles of Behavior* Hull has not gone much beyond the stage of the initial construction of the theoretical model. Except for a few scattered instances (e.g., the derivation of behavior in the simple choice situation involving differential delays of reward, the derivation of law of least work) he did not, in the *Principles*, attempt to show that his theoretical model could be employed to deduce the data of other, more complex learning situations. Two anticipations of

this type of application of Hull's theory to other learning situations than conditioning are those of Grice (3) and Thompson (11). Hull and other members of his group are at present engaged in further attempts of this type. There have been very few instances of genuine derivation of rational equations predictive of laws in the field of learning. Other outstanding examples are those of Thurstone (12) in the field of maze learning, Gulliksen and Wolffe (4) in the area of discrimination behavior and, most recently, Estes' (1) derivation of laws concerned with latency and rate measures in simple operant conditioning.

There are a number of important problems that arise in connection with the application of a theoretical model. Because of the confusion that apparently exists in this matter, I should like to mention at least one problem. The point I have in mind is the necessity in the testing of a theory for making the experimental setup, including the subjects, conform to the specifications of the theoretical model. Failure to meet this requirement precludes the possibility of drawing any worthwhile conclusions either pro or con, other than the trite one that the model is not sufficiently complete to deal with these data. Thus a theoretical model developed specifically in connection with behavior phenomena exhibited in discrimination learning of non-articulate organisms, i.e., animals, is not disproved by the failure of human subjects to behave according to the theoretical prediction. While it is true that the theory does not account for the human behavior, nevertheless, it may be a

perfectly adequate theory for the realm of phenomena for which it was intended. Unfortunately this type of "disproof" of a theory is all too prevalent in psychology.

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## FURTHER COMMENT ON APPROACH-AVOIDANCE AS CATEGORIES OF RESPONSE

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In a recent issue of this JOURNAL Weise and Bitterman (7) present data which they interpret as showing that "under certain conditions the process of discrimination cannot be appropriately described in approach-avoidance terms." In my opinion (a) their data do not justify this conclusion, and (b) their presentation tends to obscure the basic issue raised in my discussion (4) of the learned discriminative response.

The basis of the conclusion quoted above is an experiment "in which a group of rats were trained in a multiple discrimination-apparatus to choose the brighter or darker of two alleys (simultaneous problem) while a second group of rats were trained to turn in one direction when both alleys were lighted and in the opposite direction when both were dark (successive problem). The first problem proved to be significantly more difficult than the second." The authors are not explicit as to whether they consider the crucial point of their argument to be the fact that the successive problem was learned at all, or the fact that the successive problem was learned faster than the simultaneous problem.<sup>1</sup> I shall consider both points, in turn, in sections (A) and (B) below. Under (C) I shall discuss Weise and Bitterman's criticism of my numerical evidence for transfer and, under (D), the implications of some ancillary data

<sup>1</sup> Since this paper was written, Dr. Bitterman has told me that the difference in learning rates was the main basis of his conclusion. Since, as appears below, that difference has another possible explanation (see B-1) and does not, in any case, constitute a valid refutation of my position (see B-2), I am retaining the discussion of point A.

which these authors present as evidence against my position. In section (E) the non-relevance of the concept of "configurational stimuli" for the present issue is discussed, and section (F) recapitulates the basic problem regarding descriptive categories of response.

(A) *Successive discrimination learning.* The general design of the successive discrimination problem here under consideration was employed by Hunter (3), whose experiment provided the illustration for my discussion (4, p. 129): Rats were trained to enter the left path for sound X, the right path for sound Y (or for absence of sound). Weise and Bitterman used light instead of sound, different motivating conditions, and a multiple instead of a single unit apparatus, but in respect to the relevant principle their successive problem is strictly analogous. My suggestion as to how such learning fits into the approach-avoidance formulation may be restated and particularized in terms of the specific conditions which obtained in their study.

If there is any possible cue which differentiates the path on the left from that on the right, the learned response may be described as approach to one set of stimuli, avoidance of a different set. In the Weise and Bitterman study (successive problem, one subgroup), the stimuli  $S_{\text{left-path}} S_{\text{dark}}$  call for approach; the stimuli  $S_{\text{left-path}} S_{\text{light}}$  demand avoidance. The specification of  $S_{\text{left-path}}$  is crucial. It represents either one of the two types of cue suggested on page 128 of my paper: (a) spatially differentiating background features, such as a ceiling light-fixture which is slightly

to the left or right of center; (b) kinaesthetic stimuli resulting from orientation in reference to a constant landmark such as the entering pathway. In the former case, the rat would learn to approach or avoid the paths nearer and farther from the ceiling fixture, depending on whether the alley lights were turned on or off. This problem, with a type (a) cue, thus presents no difficulty for the approach-avoidance formulation.

But let us assume that no type (a) cue was available to the animals in this experiment. Our task, then, is to find a possible source of a type (b) cue. During an early trial, before learning, the rat is headed "forward" in the entering path and reaches the choice point (*cf.* 7, fig. 1). The two alleys are dark. It now can turn left and proceed, turn right and proceed, stay where it is, or retrace. We are interested only in the first two possibilities. If the animal does the first, it is rewarded (if in the last unit, by food, if in an earlier unit, by finding an unblocked path or perhaps by secondary reinforcement). If it does the second, it is not rewarded—is perhaps frustrated. In another unit, where both alleys are lighted, the first alternative is not rewarded, the second one is.

By the time the consequence (reward or no reward) occurs, the overt movement has been completed, but there may still be some reverberation or "after-discharge" from the kinaesthetic receptors. Such after-discharges, plus light-dark conditions, are the only differentiating sensory events actually coinciding with, or overlapping in time, the reinforcing (or nonreinforcing) consequences. The kinaesthetic stimulus resulting from a left turn, therefore, in conjunction with  $S_{\text{dark}}$ , can become associated (by simultaneous conditioning or association) with a forward-going or approach response, whereas in

conjunction with  $S_{\text{light}}$  it similarly becomes associated with a withdrawal or avoidance response. These associations constitute the essential learning in the problem.

What is yet to be explained is how, after learning, consistent approach or avoidance occurs *before* the actual left- or right-turn. On a later trial, the rat, coming to a darkened choice point, makes a tentative (VTE, partial, implicit) turn towards the left alley. This produces a kinaesthetic stimulus which, in conjunction with  $S_{\text{dark}}$ , has been associated with a forward-going or approach response. If the initial tentative turning is to the right, the resulting kinaesthetic stimulus in conjunction with  $S_{\text{dark}}$  elicits an avoidance response. (The differential kinaesthetic stimuli resulting from a tentative turn may be quantitatively less than those resulting from an overt turn, but they are still qualitatively distinct. The former, for instance, may derive from head-turning alone, the latter from head, trunk, tail, and limb movements.) It should be noted that the turning movement is not the "learned discriminative response" which is under discussion. Instead, the turning produces a type (b) cue, an essential component of the stimulation which elicits the learned response of approach or avoidance.<sup>2</sup>

<sup>2</sup> I should like to leave open the possibility that the cue may involve no muscle movement and no sensory consequence of that movement, but may instead come directly from a purely central "tendency" or "intention" to make a left- or right-turn.

It may be, also, that as the habit is perfected and the performance becomes "smooth," the preliminary tentative movement or tendency becomes unnecessary: the stimulus *dark* becomes independently adequate to elicit a left-turning progression. Such short-circuiting might be illustrated in the fast, unhesitant running of a thoroughly familiar maze. It should be noted that this possibility does not involve any modification in viewing the essential learning as being that of approach-avoidance.

That the animal should turn towards one or the other side (or to the left and right in alternation) is understandable enough, these being the only available alternatives of locomotion other than retracing.

For the motor learning formulation<sup>3</sup> there are two possibilities: 1) It may assume delayed reward learning, the reinforcement occurring a few seconds after the overt turning movement has been completed. 2) Or it may assume a "moving forward" of the reinforcing effects towards the choice point. Since, in this example, a type (a) cue differentiating the two pathways has been ruled out, the kinaesthetic reverberations mentioned above provide the only possible secondary or surrogate reinforcing stimuli immediately following the turn.

The assumptions underlying the approach-avoidance formulation seem no less plausible than those involved when the learned responses are described as turns to the left and right. The latter formulation has been shown (4) to be impossible in some cases, and there has been no direct refutation of the former, which offers the possibility of consistency in response-description. We turn next to the experimental findings adduced by Weise and Bitterman as indirect evidence against the approach-avoidance formulation.

<sup>3</sup> Since the criterion of discrimination involves differential movement in any case—of left- versus right-turning, or of approach versus avoidance of a given object or path—the terms "movement learning" and "motor learning" are rather ambiguous and non-definitive. The critical distinction, as I pointed out (4, pp. 128-129), is in the contrast between a muscular response, described without reference to the environment or an effect, and an act which is described in terms of a consequence (usually an altered organism-environment relationship). The former might be thought of as being on the physiological level, whereas the latter is on the psychological or behavioral level.

(B-1) *Faster learning of the successive problem.* A not unreasonable explanation for the faster learning of the successive problem in Weise and Bitterman's study is that the light-dark difference was greater and less equivocal than in the simultaneous problem. Judging from their Fig. 1 and description of the apparatus (7, p. 189), it appears that in the latter problem an appreciable amount of light must have fallen into the "dark" alley. Furthermore, the light source, being right at the choice point, did not clearly and unambiguously distinguish one alley from the other. In the successive problem, contrariwise, the choice point and the two alleys were either lighted or dark; there was little chance for reflected light from the preceding or following unit. Although the authors say that there was a "sharp difference in the brightness of the alternative pathways" in the simultaneous problem, it could not have been as great as the all-or-none difference in the successive problem. As a matter of fact, Weise and Bitterman (p. 192) themselves suggest that "If learned configurationally, the greater difficulty of the simultaneous problem may be attributed to the greater similarity of the two stimulus-patterns which it presented to the animal." Acceptance of this explanation (which is applicable whether the learning is "configurational" or involves "a more complex, higher order process"), of course, destroys the relevance of the different learning rates to the issue here under consideration. This difference was not so great that it requires two explanations.<sup>4</sup>

<sup>4</sup> Later in their discussion (pp. 193-194), the authors suggest that "if two closely similar brightness levels were employed" the simultaneous group might surpass the successive group. (Here, presumably, they are speaking of a lowered brightness difference for both simultaneous and successive situations, rather than of unequal brightness dif-



(B-2) *Learning rate and conditional discrimination.* In the concluding section of my paper I said that "the plausibility of these assumptions"—namely those pertaining to conditional reactions and, in an extreme hypothetical case, the effectiveness of "local signs" as cues—"is supported by the experimental finding that such problems are relatively difficult, and are learned slowly." That is, in order to describe the learned responses in certain discrimination problems as approach-avoidance, I was forced to assume that they were "conditional discriminations." Now in the usual meaning of the term, a conditional discrimination requires simultaneous responsiveness to two (or more) nonspatial cues. For instance: *if white and large, approach; if white and small, avoid; but if black and large, avoid; and if black and small, approach.* Under these circumstances the conditional discrimination is necessarily learned more slowly, since two sets of mutually interfering habits have to be mastered rather than only one of them.

Assuming (as Weise and Bitterman apparently do) that, when described in approach-avoidance terms, their successive problem was a conditional discrimination whereas their simultaneous problem was not, their results would appear to contradict my statement or prediction regarding relative difficulty. However, *both* of their problems involve one spatial and one non-spatial cue; neither represents conditional discrimination in the usual sense. As previously discussed (e.g., 5, p. 350), even a simple discrimination involves "conditionality" in the sense that on any one trial the alternatives are differentiated

ferences in the two problems.) The possibility of experimental evidence inconsistent with the assumption in question is thus anticipated, and an alternative explanation is provided in advance: "... opportunity for comparison . . . would offset the greater fundamental simplicity of the successive problem."

both by position (left or right) and by a visual quality such as brightness, form, or size. Both of their problems may be thought of (and therefore may "be," for the subject) conditional discriminations with one spatial and one visual cue: *If left and dark, approach; if left and light, avoid; if right and dark, avoid; if right and light, approach* (successive problem). *If left and dark, approach; if left and light, avoid; if right and dark, approach; if right and light, avoid* (simultaneous problem). As far as I know, the study of Weise and Bitterman provides the only data permitting a direct comparison of the relative difficulty of these two sets of stimulus combinations, and their results suggest that the former set is easier. Differential learning rates in these two situations, therefore, may be relevant to the problem of simultaneous versus successive presentation, but they do not refute the supporting evidence adduced in support of my assumptions.<sup>5</sup>

However, it is in large part my fault that this misunderstanding occurred.

<sup>5</sup> The two formulations may also be compared in terms of the minimum necessary number of cues in the simultaneous and successive problems: 1) For approach-avoidance, brightness alone suffices in the simultaneous problem but in the successive problem responsiveness to combinations of position and brightness cues is required. 2) For the movement formulation, brightness alone suffices in the successive problem, whereas a combination of brightness and position is necessary in the simultaneous problem. We thus have a choice, on this basis, of consistency in describing the stimulating situation, with attendant inconsistency in response-description; or of holding to one response classification with variability in "complexity" of the stimulating conditions. In this connection it may be pointed out that evidence regarding the relative "simplicity" or "primitiveness" of single-stimulus *versus* multiple-stimulus cues is by no means unequivocal. The prevalence of relational response, for instance, suggests that the patterned or configurational cue is sometimes the more "primitive" one.



My first reference to the greater difficulty of "such problems" is on page 129, top of second column (4), immediately following discussion of the aforementioned successive auditory discrimination problem. As we have seen, this problem involves one spatial and one non-spatial cue and is therefore not a conditional discrimination in the usual sense. The regrettable misplacement of my sentence may be attributed to the coincidence that, for chimpanzees, this is an extremely difficult problem (unpublished data obtained at this Laboratory). The main reason for its difficulty, I believe, is related to the sense modality involved (audition) rather than to the basic design of the discrimination learning situation.

(C) *Criteria of transfer.* Weise and Bitterman (7, p. 185) say that "perfect transfer can only be explained if we schematize the learning" in approach-avoidance terms. If that is so, one case of perfect transfer (e.g., that of Wendy who made 117 errors in learning, none in transfer) should suffice to make my point that in some cases description in approach-avoidance terms is necessary. In the 25 scores of my study, there were 13 instances of perfect transfer (no errors), and only 3 in which the savings in errors was less than 91 per cent. However, the authors go on to say that "instances of less than complete transfer deprive the approach-avoidance formulation of complete generality." This seems to me unreasonable, especially in view of the frequent observation, with chimpanzees, that a previously stable performance may be seriously disrupted by very slight changes in the context or background. (I feel confident, for instance, that if the experimenter had worn a straw hat during the "transfer tests," instead of shifting placement of the stimulus plaques from horizontal to vertical, the number of errors would have been

higher rather than lower.) The wonder is rather that the shift from horizontal to vertical (or *vice versa*) did not produce more disturbance. One might better argue that any significant amount of positive transfer is inexplicable on the basis of movement learning and thus lends support to the approach-avoidance formulation.

(D) *Nature of the evidence for approach-avoidance.* Weise and Bitterman refer to an earlier experiment (1) in which it was shown that partial differential reinforcement of a non-critical cue (consistent responsiveness to which would result in 80 per cent success) significantly influenced the learning rate of a subsequent problem in which that cue became the critical and only differentiating one. This finding is cited as being contrary to my assumptions, but just how or why the demonstration of the effectiveness of earlier experience on subsequent behavior bears on the issue is not clear.

Certainly I did not deny, directly or by implication, that animals may respond to all differentiating aspects of the stimulating situation. There is ample evidence (e.g., 6) that when two or more stimulus-aspects are differentially rewarded, the animal becomes more or less responsive to both or all of them even when responsiveness to any one of the differentiating aspects is adequate for solution. The Bitterman and Coate (1) rats evidently had learned approach-avoidance responses to both the 100 per cent and the 80 per cent reinforced stimulus aspects. How the spatial aspect (leftness and rightness) can serve as stimulus has been described above.

On page 186 of their paper, Weise and Bitterman say, "if, as Nissen suggests, the animal learns only to approach one of the two stimuli and avoid the other, their spatial relation should make no difference—a deduction which is, in fact, the basis of his own experi-

ment" (*italics mine*). Except for the italicized portions, this statement is undoubtedly correct. My animals, trained in the horizontal and tested in the vertical plane, had not been differentially reinforced in regard to *up* versus *down*. Except for possible initial preferences—innate or previously acquired—upness and downness should have "made no difference." As my transfer data show, it did not make enough difference to affect seriously the learned responsiveness to black and white.

What I did suggest, and the deduction which was, in fact, the basis of my experiment, is that "If an animal has learned a left-going response to the stimulus configuration WB, a right-going response to BW—if it has learned this and nothing more—then one would not expect consistent response to W or B when the objects are presented one above the other: W/B or B/W. If, instead, the animal has learned approach to W, avoidance of B, little disturbance would be expected when the spatial relationship is thus changed" (pp. 122–123). Unfortunately I could not train or force a group of subjects to learn only turning movements, so the first expectation could not be tested directly. The slight disturbances manifested in tests of the second expectation are attributable to change in the contextual background rather than to previous differential reinforcement of a spatial cue.

(E) *Response categories are independent of stimulus categories.* To the negative statement quoted in the first paragraph of this paper, Weise and Bitterman add the conclusion that their experimental finding "requires the assumption that the animal learns to respond differentially to discrete spatial configurations of stimuli." Gulliksen and Wolfe (2, p. 129) likewise describe the learned responses as left- and right-jumping "to the total stimulus configu-

ration consisting of two stimuli presented simultaneously in a given spatial order." To avoid confusion which might arise from this emphasis, it should be pointed out that response to "spatial configurations of stimuli" does *not* distinguish the directional movement from the approach-avoidance learning description. In either case, patterns (configurations) of stimuli, or simple (single stimulus-aspect) stimuli may be the cues. Several illustrations of approach-avoidance responses to configurational stimuli are given above and in my earlier paper (4). Thus an animal may approach the configuration *white square* (versus *white triangle*) and *black triangle* (versus *black square*). Or, the pattern may be spatial: *white over black* versus *black over white*. Finally, the configuration may encompass, or derive from, the two stimulus-objects (as in the Gulliksen and Wolfe example) instead of only one of them: when the animal approaches *black* versus *white*, it may be approaching the darker part of a white-black configuration (response to relative brightness). (Cf. 4, footnote 1, p. 123.) The issue here is not whether the stimuli are simple or configurational, spatial or nonspatial, "relative" or "absolute," but whether the response learned in the discrimination problem is approach-avoidance or a directional movement.

(F) *Motor learning versus approach-avoidance.* There is no question but that, underlying the behavior exhibited in the experimental discrimination situation, there is an available repertoire of sensory-motor coordinations which the animal uses in handling the problem with which it is now confronted (4, p. 131). In the sense of being prior in ontogenetic development, such organizations are "primitive." The process of their development should probably be looked for in the growth of

the nervous system and/or in prenatal and early neonate behavior. Whether the acquisition of such coordinations is "simpler" or "easier" than the acquisition of an approach response to a particular stimulus or stimulus-configuration (in which the underlying organizations have an instrumental function), is highly questionable. At any rate, the presently available evidence does not compel us to assume two distinct modes of learning in accounting for mammalian behavior in the discriminative choice problem.

The central problem raised in my paper (4) is this: Description and classification being the basic steps in science, choice of the descriptive categories used is of critical importance for all subsequent theorizing and search for explanatory principles. Within any one explanatory system there must be consistency in the use of classificatory terms; arbitrary vacillation from one categorization to another leads to chaos. For the broad realm of discriminative choice problems, I showed that description in terms of movement learning is, in some instances, impossible. I then tried to show that the approach-avoidance formulation is at least conceivable in all relevant cases. Success in this attempt would provide consistency on

the descriptive level for elaboration on the explanatory level. If the impossibility of description in terms of approach and avoidance were to be clearly demonstrated in any relevant instance—and so far this has not been done—we should be forced to one of the other possibilities mentioned in my earlier discussion (4, pp. 127-128).

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## DYNAMIC HYPOTHESES IN PSYCHOLOGY

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Attempts to apply a mathematics of change—for example, differential calculus—in psychology in much the same way that it has been applied to the data of physical science have not been very successful. For example, Herbart's (4) use of differential equations to describe what he believed were apperceptive processes resulted in no appreciable advances in scientific psychology. Psychological phenomena are, nevertheless, often comprised of so many dynamically interrelated variables that it would be surprising indeed if some such mathematics were not eventually to prove both applicable and useful.

Mathematical theory which is appropriate for interrelating many quantitative, continuous variables is readily available, but there are always difficulties in applying it with any rigor to psychological data. This seems to be due mainly to the problems of measurement, and possibly also to the fact that psychological variables may contain discontinuities which are poorly understood and which may therefore vitiate the ordinary methods of analysis. London (8) has been especially pessimistic concerning applications in psychology of mathematical concepts found useful in other fields.

Attempts to bring mathematical concepts into psychology are often likely to fail when such concepts suggest only hypotheses that cannot be tested empirically. Surely no one can seriously care whether or not a concept from some other field such as physics is altered drastically, or even rejected completely, provided it has in the meantime served some useful purpose in solving psychological problems.

The demand by some investigators (6, 5) for hypotheses which utilize the "dynamic" aspects of psychological data seems justified. With psychology in its present undeveloped state, hypotheses which are only as sophisticated as the classical laws of dynamics in physics might stimulate valuable research. It is not sufficiently realized that mathematical concepts now in use in most experimental psychology antedate even these classical laws and have been of little interest to physicists for over two centuries.

The early laws of motion, including the basic force equation, were first applied to a variety of simple phenomena. Whitehead (13) has described those physicists who believed that these laws could be applied to anything and everything as victims of a "fallacy of misplaced concreteness." The force equation, although valuable in describing the motion of a single mass, is totally inadequate for describing the behavior of a mass which is moving within a system of many masses. Generalizations of the force equation by Lagrange and Hamilton (11) were energy laws which were considerably more adequate for treating such dynamic systems.

Now either a group of interacting mental systems within one personality (3), a group of interacting personalities or a group of interacting social groups would certainly be more complicated than a group of interacting masses. Any laws which held for a system of masses, if at all useful for psychology, would have to be changed as experimentation dictated. The point of interest here is simply that physicists have used a Gestalt method of

treating *Gestalten*, and it might profit psychologists to do the same. Rather than attempting to treat many interacting objects individually with the "reductionistic" force equation, the mathematical physicists discovered laws for describing the whole system in terms of its set of paths and its two kinds of energy. Hamilton's principle, to be stated below, is one such law.

Hamilton's principle is not, of course, the only variational law of value in experimental science. There are many others whose virtue also lies in the defining of stationary (maximum or minimum) values of functions, whether they be energy functions, distance functions, etc. With few exceptions (5, 12, 14) psychologists have not appreciated the potentialities of the calculus of variations for constructing hypotheses that are both dynamic and testable. Two examples of such hypotheses, formulated by using mathematical concepts, will be given. One has been chosen in the field of social psychology, the other in animal psychology.

#### AN HYPOTHESIS IN SOCIAL PSYCHOLOGY

It seems very likely that members of a social group expend at least two kinds of energy in working toward their own goals and the group goals. One kind, including prestige, reputation and status, is like potential energy. The other kind, including interaction, conflict, etc., is like kinetic energy. That there is some principle similar to the conservation of energy operating in groups is suggested by the facts of social mobility, both of members within groups and of groups among other groups. That is to say, some members work actively (kinetic energy) to achieve leadership, status or prestige (potential energy) or are forced into a position of isolation (potential energy, temporarily negative in sign) by the interac-

tion and movement of the other members, but seldom remain in one state or the other for long. There is instead a waxing and waning of both types of energy, in accordance with conservative dynamic principles, both for the group members and for the group among other groups.

A first-approximation hypothesis for investigating these group phenomena would be a simple restatement of Hamilton's principle (1, 11). The latter states that,

In a system of particles subject only to their own gravitational forces, any particle will move, over a period of time, on a path such that the difference between the kinetic and potential energies of the system will be minimized.

The restatement of Hamilton's principle for investigation in social psychology would be,

An individual *S* in a social group *G* behaves, over a period of time, in such a way as to minimize the difference between

A. the ability of *G* to accomplish its work by virtue of its position, prestige, status or reputation among other groups; and

B. the ability of *G* to do its work by virtue of its interaction, conflict, etc., with other groups.

Many predictions of what *S* will do in *G*, if the restatement be true, are obvious. For example, if  $A < B$ , the members *S*<sub>i</sub> should then behave, on the average, in such ways as either to raise *A* or lower *B* or both. Or if  $A = B$ , the *S*<sub>i</sub> will behave in such ways as to preserve the balance within certain limits, etc.

#### AN HYPOTHESIS IN ANIMAL PSYCHOLOGY

The imposition of restrictions upon organismic responses is a crucial issue in modern psychology. For example, the Weber-Fechner law is transformed



into the Michels-Helson principle by imposing a set of categories into which judgments are forced (9, 10). In analytical dynamics an imposition, or "constraint," is the loss of a degree of freedom in the dynamic system. Similarly, in the mathematics of analytic fields, a "singularity" is a region where the function is restricted by having no derivatives, that is, where the function is discontinuous (1). Both of these concepts may correspond to restrictions on behavioral responses in some kind of field where learning experiments are observed. Geometrically, the singularities are like psychological barriers such as those posited by Lewin (7).

The average maze problem for a rat is a field problem into which have been introduced an excessive number of singularities or constraints. Suppose that we first imagine a rat to be unconstrained in a field and that we then impose a few psychological constraints in the form of (hypothetical) conditions necessary to direct his responses toward goals. But in addition to the presence of goals and drives let us also impose, for the sake of formulating learning hypotheses, at least two stationary conditions:

(a) The rat will approach all (perceived) goals as closely as possible on the shortest path *before* making a final goal-choice; and, simultaneously,

(b) the rat will take the shortest over-all distance (compatible with (a)) to the goal finally chosen.

With only two goals and the rat in the field, these two conditions determine the shortest network between three points, namely, the rat's starting-place  $S$  and the goals  $G_1$ ,  $G_2$  (Fig. 1). The choice-point would be at  $C$  where there are three  $120^\circ$  angles.

The  $n$ -goal problem has been considered by mathematicians and is known as "Steiner's problem" (2). The short-

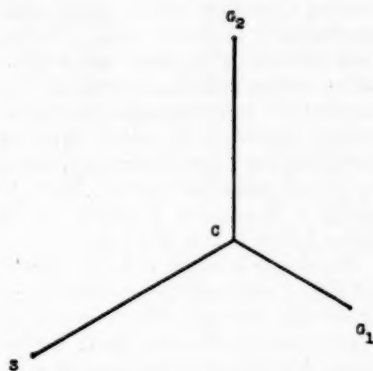


FIG. 1. Field of learning for two or three goals, assuming the operation of two variational principles.

est network between  $n$  goals is more complicated, but the principles remain the same. Probably three or more stationary conditions could be discovered which would further constrain and define the rat's field behavior.

In Fig. 1,  $C$  is the most economical point at which an organism could remain if three goals (at  $S$ ,  $G_1$  and  $G_2$ ) were to appear and then disappear randomly. The writer has not investigated whether or not a rat would actually learn to use  $C$  as a waiting point when, say, he must either go hungry or get pellets at  $S$ ,  $G_1$  and  $G_2$  before they disappeared. Such an experiment, as well as experiments using more than three goals, would be easy to perform. The possibilities of changing the number of effective goals during an experiment open a new approach to statistical learning problems.

#### CONCLUSION

Two examples of the formulation of dynamic psychological hypotheses using mathematical concepts have been presented. Such hypotheses can be tested experimentally.

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## APPROACH AND AVOIDANCE IN DISCRIMINATIVE LEARNING

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The purpose of this note is to discuss certain questions posed by Nissen's commentary (3) on a recent experiment by Weise and Bitterman (7) which was designed to study the relative difficulty of simultaneous and successive discriminative learning in the rat.<sup>1</sup> Nissen had earlier deduced (2) that successive problems must be relatively difficult for the animal, and the research in question was prompted by the fact that no systematic comparison of the two classes of problem was to be found in the literature. Nissen's deduction follows from his assumption that the process of response-selection need not be considered in the analysis of discriminative learning, and that the categories of approach and avoidance are sufficient to provide a complete account of behavior in discriminative situations. If one wishes to defend the sufficiency of these categories it becomes necessary to maintain that the mastery of the successive problem is based upon a conditional discrimination or a process of stimulus-compounding, which should make such problems relatively difficult. In the successive problem of Weise and Bitterman, for example, a rat must

learn to "approach" the compound bright-left and to "avoid" the compound bright-right; or, phrased differently, approach to bright is conditional upon leftness. Nevertheless, Weise and Bitterman found the successive problem to be considerably less difficult than the simultaneous problem.<sup>2</sup>

These results were interpreted as support for the assumption by Gulliksen and Wolfle (1) of a configurational process of discriminative learning—the assumption that under certain conditions the two pairs of stimuli function as wholes to which the animal learns to respond differentially. The relative simplicity of the successive problem pointed to a primitive perceptual process organized in terms of certain global

<sup>2</sup> New groups of animals run in the apparatus by Mr. Jack Turbeville provided results in accord with those already reported. Professor K. W. Spence (personal communication) has obtained contrary results under other experimental conditions which suggest the importance of further research designed to isolate the variables which influence the relative difficulty of the two types of problem. Results such as those reported in the initial experiment would appear to depend upon the close spatial contiguity of the two members of each pair of stimuli. The results of Saldanha and Bitterman (4) indicate that the successive problem must become extremely difficult if not impossible when closely similar stimuli are employed, since no opportunity for direct comparison of the stimuli is provided under these conditions. Professor Spence has suggested that the first results may have been influenced by a retracing effect. In the same communication he makes reference to an extension of his theory of discrimination (as yet unpublished) designed to deal with the problem of patterning which is posed by experiments such as those on successive discrimination.

<sup>1</sup> The animals were trained in a four-unit discrimination apparatus. One group was required to choose the brighter (or darker) of two alleys at each choice-point (simultaneous problem), while a second group was required to turn in one direction when both alleys were bright and in the opposite direction when both alleys were dark (successive problem). The reason for Nissen's uncertainty concerning the purpose of this experiment is not clear. The paper in which it is reported cites a number of previous studies in which the possibility of successive discrimination had been demonstrated.

properties of a stimulus-situation as distinct from a more differentiated (less readily developed) process focussed upon internal relationships. The approach-avoidance formulation, by contrast, implies that each stimulus-situation is perceptually differentiated from the very outset, and that discrimination consists only in learning which components of the situation are to be approached or avoided.<sup>3</sup> In this sense Nissen ignores the problem of perceptual development and implicitly defines discrimination as a process of response-selection—each directly differentiated afferent component is connected either to an approach or to an avoidance response. Paradoxically enough, this view is bolstered by a refusal to consider the qualitative variations in response which may occur in discriminative situations; only by emphasizing the importance of response-selection is it possible to advance beyond a response-oriented conception to an analysis of perceptual development.

The interpretation of Weise and Bitterman was tested in subsequent experiments (5, 6). Suppose that we compare the performance of two groups of animals on problems such as those schematized in Table I. The Lashley jumping apparatus is used, and both problems involve the same four stimulus-cards—two vertically striped black-and-white cards differing in stripe-thickness (*W*, wide, and *N*, narrow) and two gray cards differing in brightness (*L*, light, and *D*, dark). In Problem A each pair of cards is presented in both lateral arrangements, and the animal is rewarded for jumping to *W* and

TABLE I  
RELATIONAL AND CONFIGURATIONAL PROBLEMS

Problem		Rewarded response
A. Relational	B. Configurational	
<i>W N</i>	<i>W N</i>	Left
<i>N W</i>	—	Right
<i>L D</i>	<i>L D</i>	Right
<i>D L</i>	—	Left

*D*. In Problem B each pair of cards is presented in only one lateral arrangement, but the animal is reinforced on the same cards as in Problem A. Problem B can, conceivably, be learned in two ways—configurationally, on the basis of a *between*-pairs differentiation (as a successive discrimination), or relationally, on the basis of *within*-pairs differentiations (as a simultaneous discrimination). Since the purpose of the experiment was to find evidence of configurational perception, if such a process existed, the cards were selected in such a way that the between-pairs difference was relatively large with respect to within-pairs differences.<sup>4</sup> Since no conditional discrimination is required by Problem B—that is to say, no linkage between visual and positional cues is required for its solution—a simple approach-avoidance theory leads to the prediction that the two problems should be functionally equivalent. In both cases the animals should learn to approach *W* and *D* and to avoid *N* and *L*; the two problems should be learned at the same rate and there should be perfect transfer from each to the other. Actually, however, Problem B is mastered more rapidly, and, even after

<sup>3</sup> "The integrations which provide . . . perceptions . . . of things, of direction, distance, and so on . . . are either innate or have been acquired in earlier ontogeny," writes Nissen. "With the origins of these basic organizations we have not here been concerned; we have taken these units of integration for granted" (2, p. 131).

<sup>4</sup> Weise and Bitterman assumed on the basis of their results that the configurational organization is preferred even when within- and between-pairs differences are equal, provided that both are large.

considerable overtraining, there is little transfer to Problem A. That the two-situational problem is organized configurationally is shown by the fact that, confronted with situations *NW* and *DL* for the first time in Problem A, animals that have mastered Problem B jump consistently to *N* and *L*, and overtraining on Problem B increases this tendency. Apparently the animals trained on the two-situational problem learn to respond differentially in terms of a gross difference between the striped and gray pairs, and there is little tendency for these pairs to become internally differentiated in the course of training.

These data present much the same sort of difficulty for approach-avoidance theory as do those of the initial experiment. In both cases new assumptions about stimulus-compounding or conditional discrimination are required to bolster the theory. Nissen (3, and personal communication) maintains that it is necessary to distinguish between conditional discriminations involving two visual components and those involving one visual and one spatial component. While compounds of the first kind, which are established only in special discriminative problems, are difficult to develop, Nissen seems to suggest that visual-kinesthetic compounds, which are inherent in every discriminative problem (since the visual stimulus must occupy a position in space), may emerge very readily.

Although this formulation accounts for the relative difficulty of simultaneous and successive problems which was found in the initial experiment, and for the relative difficulty of the two- and four-situational problems studied in subsequent experiments, there are certain difficulties still to be faced. For example, if an animal is trained in Problem B (Table I) to jump left in situation *WN*—in Nissen's terms, to

"approach" *W-left*—why, when it encounters *NW* for the first time in Problem A, does it consistently approach *N*? Special assumptions must be introduced to deal with the apparent dominance of the kinesthetic component in the determination of the approach response. Furthermore, if visual-kinesthetic conditionality (compounding) automatically develops in the course of training on conventional problems which involve the lateral reversal of each pair of stimulus-cards, why did any of Nissen's animals (2) show perfect transfer from right-left to up-down stimulus arrangements, or the reverse, when different compounds presumably were involved?<sup>8</sup> Finally, it may be asked how approaches to stimuli occupying different positions in space generate discriminably different kinesthetic cues, if the responses themselves are regarded as identical.<sup>9</sup>

<sup>8</sup> The simple approach-avoidance formulation requires perfect transfer in every case while the compounding formulation which, as we have seen, must assign special weight to kinesthetic components, cannot deal with perfect transfer. Nissen (3) reacts with some indignation to the suggestion of Weise and Bitterman that the lack of perfect transfer in his experiment deprives the simple approach-avoidance theory of complete generality, yet he himself lists "shift in the habitual motor pattern" (2, p. 123) as one of the factors which may be responsible for incomplete transfer.

<sup>9</sup> The extremes to which Nissen is prepared to go in defense of the approach-avoidance formulation may be seen in his discussion of a hypothetical problem which requires the activation of two admittedly distinct motor systems (limb and facial), each by a different stimulus (2, p. 130). The assertion that both responses involve muscle "twitches," a vague reference to "local signs" which are assumed to be conditionally related to the discriminanda, and a change in the meaning of the symbols *R+* and *R-* (previously defined as approach and avoidance responses but now implying something like excitation and inhibition), are apparently regarded as sufficient for the resolution of all problems.

Nissen's insistence upon an approach-avoidance formulation is, we are told, motivated by a desire for consistency in the use of descriptive categories, and he implies that the only alternative is inconsistency—"arbitrary vacillation from one categorization to another." The real issue is, of course, not clarity versus confusion, but simplicity versus complexity. Parsimonious categorization is desirable, but the principle implies that justice must be done to the complexity of the events being described. It is possible at the present time to regard the approach-avoidance conception as an oversimplification of the problem of discrimination and to look toward the development of a theoretical framework which will bring us closer to the realities of perceptual organization.

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